

EXHIBIT 37

Confidential

Wade Miquelon

In re Arizona Theranos, Inc. Litigation

1 IN THE UNITED STATES DISTRICT COURT

2 FOR THE DISTRICT OF ARIZONA

3 IN RE:

)

4) Civil Action No.

5 THERANOS INC.,

) No. 2:16-cv-2138-HRH

6 LITIGATION,

)

7
8
9 *** CONFIDENTIAL ***

10
11 VIDEOTAPED DEPOSITION OF WADE MIQUELON

12 August 9, 2019

13 Chicago, Illinois

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23 REPORTED BY:

Sheri E. Liss,

24 CSR, RPR, CRR, CLR

25 JOB NO. 10057960

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1 meeting, it doesn't mean everybody's in the same
2 meeting.

3 Q. As far as you know, did Mr. Hunter have
4 access to all of the information that he needed from
5 Walgreens in order to make his assessment of
6 Theranos?

7 MR. LEVINE: Foundation.

8 BY THE WITNESS:

9 A. I can't judge that. How would I know?

10 BY MS. HOWARD:

11 Q. Are you aware of anyone ever denying a
12 request made by Mr. Hunter for information?

13 A. I am not aware of that.

14 Q. At any point did Mr. Hunter ever advise
15 Walgreens that he did not think it was possible for
16 the Theranos technology to work as claimed?

17 A. He certainly didn't tell me or anybody
18 that I know.

19 Q. Would you have expected him to tell you
20 something like that if that was his belief?

21 A. I mean, I would just say that I work
22 with so many consulting firms throughout the years,
23 at least the ones that are credible, if they see an
24 issue, they raise it. That's why you pay them. But
25 I don't even know the man so I don't know what he

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1 thinks, but for sure he never came to me and raised
2 an issue to me.

3 Q. Do you have an understanding as to how
4 long Mr. Hunter was a consultant for Walgreens?

5 A. I do not.

6 Q. At some point have you reviewed the
7 reports that Mr. Hunter generated on behalf of
8 Walgreens?

9 A. I don't know what those reports are. I
10 remember one report that had the circa 180 labs on a
11 chip company.

12 Q. Can you repeat that?

13 A. I think it was a report that was done by
14 his firm that was basically assessing the landscape
15 of the 180 or so companies in the lab space or lab
16 on a chip or point of care lab space, that's the
17 only document that I can remember by that firm.
18 There could have been others.

19 Q. Do you know one way or another if Mr.
20 Hunter continued to provide consulting services to
21 Walgreens after he issued those reports?

22 A. I do not know.

23 Q. Do you know who he would have likely
24 worked with at Walgreens in his consulting capacity?

25 A. Those names that I mentioned before.

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1 **Q. Such as?**

2 A. Mr. Rosan, perhaps Mr. Finnegan at that
3 time, Colin Watts.

4 **Q. Are you aware of accounts that Mr.**
5 **Hunter has given to the media about the in-person**
6 **meeting that he says he attended at Theranos with**
7 **Walgreens executives?**

8 A. Not specifically, no.

9 **Q. Have you read the book Bad Blood?**

10 A. I have not.

11 **Q. Have you watched or listened to the ABC**
12 **news reporting or podcast regarding Theranos called**
13 **The Drop Out?**

14 A. I've heard part of The Drop Out. I was
15 listening to it. My wife was listening to it, and I
16 overheard some of it.

17 **Q. Do you recall overhearing the part where**
18 **Mr. Hunter was interviewed?**

19 A. I don't know if it was from that or not
20 but I caught some snippet at some point of him.

21 **Q. You don't have a specific recollection**
22 **of Mr. Hunter being interviewed in that podcast?**

23 A. Whatever I saw externally, I don't know
24 if it was TV or podcast or something snippety, he
25 was essentially saying he saw the train wreck

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1 coming. He was the only one. He knew all of this,
2 whatever. I was pretty disgusted by the whole
3 thing, if you want to know the truth.

4 **Q. Why was that?**

5 A. Because if he did, he should have said
6 something. Not just because he was a client and had
7 a fiduciary responsibility to tell us, but because
8 he's allegedly a professional lab. And if you think
9 a company is about to do something that could
10 potentially harm people, just from a moral point of
11 view you should say something.

12 **Q. Again just to clarify what you said**
13 **earlier, Mr. Hunter never told you or anyone else to**
14 **your knowledge that he believed the Theranos**
15 **technology didn't work, correct?**

16 A. Never told me, no one to my knowledge.

17 **Q. He never told you or anyone else at**
18 **Walgreens that Walgreens should not move forward**
19 **with the Theranos partnership, correct?**

20 MS. GARDNER: Object to form.

21 BY MS. HOWARD:

22 **Q. You may answer.**

23 A. Not to my knowledge, no.

24 **Q. Do you recall at the meeting I**
25 **referenced earlier, which I'll represent to you**

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1 happened in August of 2010, that Mr. Hunter
2 attended, or at least said he attended in his
3 interviews, do you recall being presented with a
4 flag by Elizabeth Holmes?

5 A. I have a recollection of a flag that --
6 or a note that she sent to Greg and I about a flag
7 that was going to fly over DC or something and be
8 presented to us.

9 Which at the time we thought, you
10 know, with all the work with the military and
11 whatever, this is amazing.

12 I don't know -- I seem to remember
13 being presented with that flag, but I don't know if
14 that was -- I don't remember if that was in
15 Deerfield or in Palo Alto. I don't remember.

16 Q. You don't remember if she gave you the
17 flag on-site at Theranos or it was sent to you at
18 some later date?

19 A. I don't have that flag. Greg might know
20 the answer to that. He probably would have kept it
21 in the archives or something at Walgreens, but --

22 Q. I'll have you mark this 285 and 286.

23 (Whereupon, Exhibit 285 and
24 Exhibit 286 marked as
25 requested.)

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1 (Whereupon, the documents were
2 tendered.)

3 BY THE WITNESS:

4 A. This rings a bell.

5 BY MS. HOWARD:

6 Q. The court reporter has just marked
7 Exhibit 285, which is an email from Elizabeth Holmes
8 to a number of people, including Dr. Rosan. It
9 states, "Dear all, on the Monday after we formalized
10 our partnership, we had a U.S. flag flown off the
11 Capitol in D.C. in honor of Walgreens and its
12 employees. I have attached a copy of the
13 accompanying certificate to this email."

14 Do you see that?

15 A. I do.

16 Q. And then the last page of the
17 attachment -- or excuse me -- of the exhibit should
18 be a certificate.

19 A. Yes.

20 Q. And the certificate page has a Bates
21 number WAG-TH ending in 9012, certificate signed by
22 the Architect of the Capitol. "This is to
23 certificate that the accompanying flag was flown
24 over the United States Capitol on August 2, 2010.
25 At the request of Honorable Mark S. Critz, Member of

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1 Congress, this flag was flown by Theranos in honor
2 of Walgreens and its employees for their commitment
3 to improving the health and well-being of all
4 Americans."

5 Does this refresh your recollection
6 about the flag gift?

7 A. Yes.

8 Q. Now turn to the exhibit that's marked
9 286. Exhibit 286 is Bates number WAG-TH-61032.
10 It's an email from you to Colin Watts, CC Dan Doyle,
11 Elizabeth Holmes and a few others. I apologize, the
12 writing is extremely small. You may need a
13 magnifying glass to read it.

14 You write to Elizabeth, "You are a
15 class act all the way and I can only echo Colin's
16 sentiments by saying that this is one of the most
17 exciting opportunities from both a business point of
18 view and doing good point of view I have ever been a
19 part of. Really looking forward to an amazing
20 partnership with your team well into the future."

21 Is that an accurate reflection of
22 your sentiments in August of 2010?

23 A. It was at that time.

24 Q. Are you aware of any other flags that
25 have been presented to you or anyone else at

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1 Walgreens by Ms. Holmes?

2 A. No.

3 Q. Would it surprise you to hear that Mr.
4 Hunter in media interviews has claimed when Ms.
5 Holmes presented the flag to you directly, Mr.
6 Miquelon, that she said the flag was flown over the
7 battlefield of Afghanistan?

8 A. That would surprise me.

9 Q. Would it surprise you because that
10 didn't happen?

11 A. I most certainly don't recall that.

12 Q. What you recall is receiving a flag from
13 Ms. Holmes that was stated to be flown over the U.S.
14 Capitol as reflected in the certificate that you
15 reviewed, is that accurate?

16 A. That's my recollection.

17 Q. You can put those exhibits aside.
18 Did you ever have your own blood
19 tested on a Theranos device?

20 A. Twice.

21 Q. When was that?

22 A. I think twice. Everything is I think.
23 It's been so long ago.

24 Early in the process I had my
25 finger tested at Walgreens. And then later in the

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1 I further certify that I am not counsel
2 for nor in any way related to any of the parties to
3 this suit, nor am I in any way interested in the
4 outcome thereof.

5 I further certify that this certificate
6 applies to the original signed and certified
7 transcripts only. I assume no responsibility for
8 the accuracy of any reproduced copies not made under
9 my control or direction.

10 IN TESTIMONY WHEREOF I have hereunto set
11 my hand this 19th day of August, 2019.

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18

19 Sheri E. Liss, CSR, RPR, CRR, CLR
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EXHIBIT 38

Message

From: Wayne.Prince@Walgreens.com [Wayne.Prince@Walgreens.com]
Sent: 9/29/2010 3:10:26 PM
To: Daniel Young [/o=theranos organization/ou=first administrative group/cn=recipients/cn=dyoung]
CC: Renaat.VanDenHooff@Walgreens.com; Sunny Balwani [/o=theranos organization/ou=first administrative group/cn=recipients/cn=sbalwani]
Subject: RE: today's demo

Sweet! Then all is well in the world.

Wayne Prince, PMP
Project Manager, Business Process Re-engineering
Corporate Innovation Team
847.964.8973 (w)
(c)
<http://www.linkedin.com/in/wayneprince>

"Daniel Young" <dyoung@theranos.com>
09/29/2010 10:09 AM

To <Wayne.Prince@Walgreens.com>
cc "Sunny Balwani" <sbalwani@theranos.com>,
<Renaat.VanDenHooff@Walgreens.com>
Subject RE: today's demo

Yes, we have heartbeats for devices 347 and 342.

Thanks,
Daniel

From: Wayne.Prince@Walgreens.com [mailto:Wayne.Prince@Walgreens.com]
Sent: Wednesday, September 29, 2010 7:57 AM
To: Daniel Young
Cc: Sunny Balwani; Renaat.VanDenHooff@Walgreens.com
Subject: RE: today's demo

Daniel,

Ok, I set up all the machines.

All have booted and assumed optimal operating temperature.

I removed the cartridge from 313. We made sure to transport it in the vertical position. I have since powered it down and boxed it up as that is the one we will ship back to you.

Do we have heartbeats on the other two?

Thanks,

Wayne Prince, PMP
Project Manager, Business Process Re-engineering
Corporate Innovation Team
847.964.8973 (w)
(c)

<http://www.linkedin.com/in/wayneprince>

"Daniel Young" <dyoung@theranos.com>

09/29/2010 09:08 AM

To <Wayne.Prince@Walgreens.com>
cc "Sunny Balwani" <sbalwani@theranos.com>
Subject RE: today's demo

I'm glad it was a success. Thanks for keeping us updated.

-Daniel

From: Wayne.Prince@Walgreens.com [mailto:Wayne.Prince@Walgreens.com]
Sent: Wednesday, September 29, 2010 7:04 AM
To: Daniel Young
Cc: Sunny Balwani
Subject: Re: today's demo

Daniel,

The kickoff was a success.

The demos went fine. We shut the machines down in the middle of the assay. We spoke to Sunny about this last night. We had to move the machines back to our building that night and we didn't want to wait for the assays to complete and the business day was over.

Our expectation is that we will not get any usable results from the assays that were started late yesterday afternoon.

I am going to set the machines back up now. I will contact you once they seem to be ready to confirm connectivity.

Thanks,

Wayne Prince, PMP
Project Manager, Business Process Re-engineering
Corporate Innovation Team
847.964.8973 (w)

(c)

<http://www.linkedin.com/in/wayneprince>

"Daniel Young" <dyoung@theranos.com>

09/28/2010 06:08 PM

To <Wayne.Prince@Walgreens.com>

cc

Subject today's demo

Hi Wayne,

I hope the kickoff was a success and that the demo went smoothly for you. We have noted that device 342 has had spotty reception since the change in location today and we are concerned that all the data may not get transferred from this afternoon. Is it possible to power this device "on" again in the old location to ensure that all the data is transferred? If so, please let me know when you plan to do this.

Thanks!

Daniel Young, PhD
Director, Theranos Operating System and Computational Biosciences
Theranos, Inc
3200 Hillview Avenue
Palo Alto, Ca 94304
650-470-6119

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650-838-9292 www.theranos.com

EXHIBIT 39

Message

From: Sunny Balwani [/O=THERANOS ORGANIZATION/OU=FIRST ADMINISTRATIVE GROUP/CN=RECIPIENTS/CN=SBALWANI]
Sent: 10/13/2010 3:39:29 PM
To: Daniel Young [/o=theranos organization/ou=first administrative group/cn=recipients/cn=dyoung]
Subject: Fwd: Hey, I just did a demo with a VIP and I need the results as soon as they are done.

Fyi

Sent from my iPhone

Begin forwarded message:

From: Jay.Rosan@Walgreens.com
Date: October 13, 2010 5:49:07 AM PDT
To: Sunny Balwani <sbalwani@theranos.com>
Subject: **Re: Hey, I just did a demo with a VIP and I need the results as soon as they are done.**

Sunny, that is great but there is still one that I have not received 347.101011.02
Dr. Jay Rosan
VP Health Innovation
Walgreens
[REDACTED]
jay.rosan@walgreens.com

From: Sunny Balwani <sbalwani@theranos.com>
To: Jay Rosan <Jay.Rosan@Walgreens.com>
Date: 10/12/2010 07:43 PM
Subject: Re: Hey, I just did a demo with a VIP and I need the results as soon as they are done.

Dr. jay. We have automated this so now you don't need to send me emails for this. Daniel gets automotified and once our CMO has looked over the results, you will get the results automatically. If it doesn't work, you are always welcome to shoot me an email of course.

Thanks.

On 10/12/10 6:42 AM, "Jay Rosan" <Jay.Rosan@Walgreens.com> wrote:

Tx - how do we make this just automatic so that I don't have to bother you for results - I did another one later in the day.
Dr. Jay Rosan
VP Health Innovation
Walgreens
[REDACTED]
jay.rosan@walgreens.com

From: Sunny Balwani <sbalwani@theranos.com>
To: Jay Rosan <Jay.Rosan@Walgreens.com>
Date: 10/11/2010 02:26 PM
Subject: RE: Hey, I just did a demo with a VIP and I need the results as soon as they are done.

Dr. Jay

Today's result was added to the attached summary. PASSWORD unchanged.

Thanks.
SRB.

From: Sunny Balwani
Sent: Monday, October 11, 2010 10:42 AM
To: Daniel Young
Cc: Elizabeth Holmes
Subject: Fwd: Hey, I just did a demo with a VIP and I need the results as soon as they are done.

Daniel. Can u get the results and email to me asap. Thanks

Sent from my iPhone

Begin forwarded message:

From: <<mailto:Jay.Rosan@Walgreens.com>> <<mailto:Jay.Rosan@Walgreens.com>> >
Jay.Rosan@Walgreens.com <Jay.Rosan@Walgreens.com>
Date: October 11, 2010 7:22:35 AM PDT
To: Sunny Balwani <sbalwani@theranos.com> <sbalwani@theranos.com> >
Subject: Hey, I just did a demo with a VIP and I need the results as soon as they are done.

I would tell you the name but that would not be right but it is one of the highest individuals in the company. He is expecting me to walk in with the results within the hour.

J

Dr. Jay Rosan
VP Health Innovation
Walgreens


jay.rosan@walgreens.com <jay.rosan@walgreens.com>

----- End of Forwarded Message[attachment "Demo Data 10-11-2010.pdf" deleted by Jay Rosan/Corp/Walgreens]

EXHIBIT 40

*Damon M. Petrich, Travis C. Pratt, Cheryl Lero Jonson,
and Francis T. Cullen*

Custodial Sanctions and Reoffending: A Meta-Analytic Review

ABSTRACT

Beginning in the 1970s, the United States began an experiment in mass imprisonment. Supporters argued that harsh punishments such as imprisonment reduce crime by deterring inmates from reoffending. Skeptics argued that imprisonment may have a criminogenic effect. The skeptics were right. Previous narrative reviews and meta-analyses concluded that the overall effect of imprisonment is null. Based on a much larger meta-analysis of 116 studies, the current analysis shows that custodial sanctions have no effect on reoffending or slightly increase it when compared with the effects of noncustodial sanctions such as probation. This finding is robust regardless of variations in methodological rigor, types of sanctions examined, and sociodemographic characteristics of samples. All sophisticated assessments of the research have independently reached the same conclusion. The null effect of custodial compared with noncustodial sanctions is considered a “criminological fact.” Incarceration cannot be justified on the grounds it affords public safety by decreasing recidivism. Prisons are unlikely to

Damon M. Petrich is a criminal justice PhD candidate at the University of Cincinnati; Travis C. Pratt is research director for the Harris County Community Supervision and Corrections Department and a research fellow at the University of Cincinnati Corrections Institute; Cheryl Lero Jonson is associate professor of criminal justice at Xavier University; Francis T. Cullen is Distinguished Research Professor Emeritus of Criminal Justice and senior research associate at the University of Cincinnati.

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reduce reoffending unless they can be transformed into people-changing institutions on the basis of available evidence on what works organizationally to reform offenders.

After a nearly 600 percent increase in the rate of imprisonment in the United States between 1972 and 2007 (Pager 2007; Nellis and King 2009), this trend is slowly beginning to reverse, with rates declining by an average of 1.2 percent per year between 2008 and 2018 (Maruschak and Minton 2020). Still, the latest nationwide data compiled by the Bureau of Justice Statistics (Maruschak and Minton 2020) show that 2,123,100 Americans were held in federal or state prisons and local jails as of December 2018. Even with declines over the past decade, the United States maintains its dubious title as the world leader in imprisonment with approximately 655 prisoners per 100,000 citizens, followed by El Salvador (604) and Turkmenistan (526). The imprisonment rates of other culturally comparable countries, such as Canada (114), the United Kingdom (140), and Australia (172), are but a fraction of America's (World Prison Brief 2018). Taken together, government agencies in the United States spend approximately \$80 billion per year on corrections (National Academy of Sciences 2014), with average incarceration costs of around \$30,000 per inmate per year (PEW Center on the States 2009; Vera Institute 2017).

Scholars have produced myriad works over the past three decades that attempt to account for the meteoric growth of incarceration in the United States (see, e.g., Garland 2001; Zimring 2001; Tonry 2004, 2007, 2009; Gottschalk 2006; Clear and Frost 2014; Enns 2016; Pfaff 2017, 2020; Muhammad 2019). Within these works, a diverse range of factors have been proposed as holding explanatory value: increased rates of crime beginning in the 1960s; beliefs that rehabilitation programs for offenders do not work; the resurgence of conservative ideals such as individual responsibility, commonsense law and order, and absolutist conceptions of right and wrong; racial resentment and the use of crime policy to protect against minority encroachment on White social and economic advantage; mass media sensationalism of rare yet high-profile correctional system failures; increases in fear of crime and expansions of victims' advocacy groups; the existence of conflictual, two-party political systems; and democratic rather than meritocratic selection of prosecutors and judges. Any one of these factors is unlikely to be a sufficient explanation, some factors hold more merit than others, and many factors may have interacted to nourish the nation's protracted experiment in mass incarceration (Tonry 2004; Pfaff 2020).

Regardless of the motivating rationales and causes, a host of legal and policy changes between the mid-1970s and 1990s made sentences to imprisonment much more likely and longer. From the mid-1970s onward, states such as California, Illinois, Indiana, and Maine introduced determinate sentencing reforms that severely limited or completely abolished the use of parole. Laws focused on creating mandatory minimum sentences also proliferated the country between the 1970s and 1990s. For example, Michigan enacted the “650 Lifer Law” in 1978, which originally mandated life without parole if convicted of trafficking more than 650 grams of cocaine or heroin. Likewise, the 1984 federal Comprehensive Crime Control Act required a five-year sentence enhancement for carrying a firearm during the commission of another drug or violent offense. In the 1990s, more than half of the states passed some form of a three strikes or habitual offender law in attempts to target repeat offenders. In California, for instance, these laws required a life sentence for individuals convicted of a felony who had been convicted of two or more felonies in the past. At the federal level, too, legislation such as the 1994 Violent Crime Control and Law Enforcement Act provided states with additional funding for the construction of prisons, conditional on evidence that the state was sentencing more violent offenders to prison and for longer (Zimring, Hawkins, and Kamin 2001; Spohn 2008; Tonry 1996, 2009; National Academy of Sciences 2014; Pfaff 2017).

It is often overlooked that the escalation of punitive policies was detached from any empirical base of knowledge regarding the effects of imprisonment on crime. Rather, these measures were largely based on “blind faith that a silver-bullet solution [could] magically solve the [crime] problem” (Mears and Cochran 2015, p. 58). Some theoretical discussion and research attempted to assess the notion that crime could be reduced by incapacitating active offenders via imprisonment (Shinnar and Shinnar 1975; Blumstein, Cohen, and Nagin 1978; Spelman 1994, 2000). However, little empirical work was done to assess the effects of custodial sanctions on post-release reoffending (Mears and Cochran 2015). Based on our survey of this literature, only 13 studies comparing the outcomes of those sentenced to custodial and noncustodial sanctions were conducted prior to 1990, another 23 in the subsequent decade, and another 25 through to 2008. Many of these early studies were of conspicuously poor methodological quality, often relying on bivariate assessments or on regression and exact matching models that adjusted for only a handful of relevant confounders (Nagin, Cullen, and Jonson 2009; Villettaz, Gillieron, and Killias 2015). In short, during the times when the mass incarceration movement began and grew most rapidly,

relatively little was known about the effect being imprisoned had on reoffending.

Lacking an empirical foundation for their experiment with large-scale imprisonment, politicians justified their efforts with the “common sense” notion that people are rational actors and could therefore be deterred from crime if punishments were severe enough (Garland 2001; Clear and Frost 2014). As imprisonment is commonly viewed as the ultimate loss of freedoms, social connection, and the like, increasing the likelihood of being sent to prison and the length of prison terms was viewed as a uniquely effective way to deter offending. This view had some support among economists, political scientists, and other scholars (Becker 1968; Wilson 1975; van den Haag 1977), though their support was generally based on theory rather than empirical evidence. An alternative perspective, advanced by the majority of sociologists and criminologists, was that imprisonment is more likely to have criminogenic effects than to serve as an effective deterrent to prisoners (e.g., Shaw 1930; Sutherland 1939; Sykes 1958; de Tocqueville [1844] 1968; Braithwaite 1989). These scholars suggested that a variety of factors associated with a custodial sentence increase reoffending, including close-quarters cohabitation with and learning from other inmates, the strains of exposure to violence and loss of personal freedoms, the fraying of ties to support networks, and the collateral consequences attached to a criminal conviction such as barriers to employment and housing. Until fairly recently, however, direct evidence showing the criminogenic effect of these conditions has remained limited (see, e.g., Sampson and Laub 1993; Bayer, Hjalmarsson, and Pozen 2009; Listwan et al. 2013).

To have embarked on a decades-long experiment in mass incarceration with scant empirical knowledge of the effect of prison on postrelease offending seems inexplicable in retrospect. On any given day, more than two million people are held in correctional institutions in the United States (Maruschak and Minton 2020). Ninety-five percent of inmates are eventually released, which means that approximately 600,000 individuals leave prisons each year and another 10.7 million cycle through local jails (Carson 2020; Zeng 2020). With so many lives at stake—including both prisoners and potential victims—correctional policy should be informed by research. That is why a comprehensive review of the research on custodial sanctions and reoffending is necessary. We provide that review. Prior narrative and meta-analytic reviews have indicated that, compared with noncustodial sanctions such as probation, sentencing individuals to terms of imprisonment generally has a null or criminogenic effect on reoffending (Nagin,

Cullen, and Jonson 2009; Jonson 2010; Villettaz, Gillieron, and Killias 2015). However, these reviews are limited by factors such as small numbers of primary studies to draw from, few studies of strong methodological rigor, modeling strategies that could not account for multiple effect sizes nested within individual studies, and limited or nonexistent analyses of factors that might moderate the effects of custodial sanctions on reoffending.

The meta-analytic review presented in this essay overcomes these problems and builds on earlier work in four important ways. First, it includes 55 additional studies produced since the last comprehensive reviews were made by Nagin, Cullen, and Jonson (2009) and Jonson (2010). Taken together, these more recent studies include 691 effect size estimates, many from models that capitalize on natural experiments or use propensity score methods. Second, we expanded our inclusion criteria beyond those of Villettaz, Gillieron, and Killias (2015) to include all studies comparing custodial and noncustodial sanctions, including those using bivariate and multivariate regression analyses. Third, we capitalize on advances in meta-analytic techniques to use a multilevel modeling approach (Hox, Moerbeek, and van der Schoot 2018). These methods allow individual effect sizes to be weighted by their precision rather than sample size and allow for the inclusion of multiple effect sizes from within individual studies while accounting for their statistical dependence. Fourth, given the large number of available effect size estimates ($N = 981$) and a detailed coding scheme, we were able to assess the influence of a broad range of potential moderators, including the overall research design, covariates accounted for in statistical models, types and lengths of sanctions examined, and sociodemographic characteristics of samples such as the age and gender distributions. Following Nagin, Cullen, and Jonson (2009, p. 120), we “use the concept of reoffending to refer to all criminal acts committed by a person following a legal sanction”—in this case, a custodial or noncustodial placement. As Section IV shows, custodial sanctions have a null or criminogenic effect on reoffending when compared with noncustodial sanctions such as probation. Although a small number of factors moderate effect size estimates (e.g., research design, type of reoffending measure), we find no conditions under which custody reduces reoffending.

This subject is particularly timely given there are signs that America’s fixation on imprisonment is beginning to wane (Petersilia and Cullen 2015; Tonry 2019; Butler et al. 2020). Data from the Bureau of Justice Statistics show that prison and jail incarceration rates have declined by 15 percent and 11 percent, respectively, since 2008 (Carson 2020; Zeng 2020). There

is also now bipartisan support for efforts to reform the criminal justice system. At the federal level, for example, the Second Chance Act and the First Step Act were collaborative efforts by Democrats and Republicans and signed into law by Presidents George W. Bush and Donald J. Trump (Lattimore and Visser 2009; Cohen 2019). These acts have provided funding to create and evaluate programs aimed to facilitate offenders' reentry into their communities (e.g., education and employment assistance, cognitive-behavioral treatment), develop risk assessment tools, investigate sentencing reforms, and develop partnerships with community organizations (for a review of the effectiveness of these efforts, see Petrich et al. 2021). Efforts to reduce prison populations are also occurring at the state level, with previously high-incarceration states such as Louisiana and Mississippi exploring justice reinvestment policies and changes to sentencing guidelines (Gray 2011; Cohen 2017; PEW Charitable Trusts 2018). Finally, there is an increasing amount of public support for and political and media discourse about issues such as ban-the-box, problem-solving courts, felon disenfranchisement, criminal record expungement, and rehabilitation ceremonies (e.g., Love and Schluskel 2019; Thielo et al. 2019; Butler et al. 2020).

Two other considerations are germane. First, more attention is being paid to the racial disparities that pervade the American criminal justice system, owing in large part to public outrage over recent tragic events such as the killings of Eric Garner, George Floyd, and Breonna Taylor. Because of the role of law enforcement in these high-profile killings, much ensuing policy discussion has centered around proposals to defund the police in one way or another, whether through complete abolishment or redirecting funds toward social work, mental health, and substance abuse treatment (Lowrey 2020; North 2020; Searcey 2020). But it is important also to consider that disparities exist beyond police-citizen interaction (Ba et al. 2021). Influential books such as Michelle Alexander's *The New Jim Crow*, documentary films such as *13th* (DuVernay 2016), and myriad empirical analyses show that disparities in the correctional system are often large and have long-term negative consequences for Black communities (Clear 2007; Kirk 2016). Studies have consistently shown that Black citizens, particularly young males, are more likely than Whites to be placed in pretrial detention, receive sentences of incarceration, and receive longer sentences (e.g., Bales and Piquero 2012b; Wooldredge et al. 2015; Holmes, Feldmeyer, and Kulig 2020; see also King and Light 2019). In light of these disparities, it is important to think about how the mass application of a sanction that does

little to reduce reoffending will negatively affect citizens and their communities. Outcomes commonly cited in the literature include the dissolution of families, depression of local economies, and greater cynicism toward the criminal justice system (Clear 2007; Western and Wildeman 2009; Kirk 2016). If sentences such as probation produce reoffending outcomes comparable to those of imprisonment and simultaneously alleviate collateral consequences, the current public policy debates about criminal justice reform should take this into account.

Also germane are the effects of the coronavirus pandemic on prisoners, their release, and crime; 37.7 million people in the United States had contracted COVID-19 as of August 23, 2021, of whom 628,285 died (“Coronavirus in the U.S.” 2021). Given the close-quarters nature of the prison environment, it is not surprising—though disturbing—that 661,000 inmates and employees in America’s jails and prisons have been infected and another 2,990 have died (“Coronavirus in the U.S.” 2021). These are much greater rates of infection than in the population-at-large. For example, a report by Schwartzapel, Park, and Demillo in December 2020 showed that the rate of infection among US prisoners was four times greater than among the general population. In some states, for example Kansas, infection rates among prisoners were as much as eight times larger. In response to these troubling numbers, many state and local legislators have taken actions that led to the release of small numbers of inmates who were close to their scheduled discharge dates (Porter 2021; Prison Policy Initiative 2021). However, although prison and jail populations dropped by 9 percent and 24 percent, respectively, between 2019 and 2020 (Kang-Brown, Montagnet, and Heiss 2021), most of these changes appear to be the result of reduced admissions rather than increased releases (Widra and Wagner 2020). The reoffending outcomes of those released from incarceration remain to be seen. Our own review of the literature on custodial sanctions shows that those sentenced to custody are equally as likely to reoffend as those who remain on community supervision. This raises concerns about the small number of releases in the midst of battling a virus that spreads rapidly in confined, close-quarters spaces (Vose, Cullen, and Lee 2020).

Against the backdrop of declines in prison populations over the past decade, growing support for exploring alternatives to incarceration, concerns over racial disparities, and the close-quarters confinement of large numbers of inmates during a pandemic, there is need for a comprehensive empirical understanding of the role of incarceration in reducing reoffending and promoting public safety. Here is how this essay, which addresses that need, is

organized. Section I examines two competing perspectives on the effects of incarceration. The deterrence perspective holds that punishment via imprisonment changes the decision-making processes of offenders and diminishes their desire to reoffend. The criminogenic perspective holds that offenders are exposed to a range of experiences during and after imprisonment that make reoffending more likely. Section II discusses the current state of research examining these perspectives, including reasons for heterogeneity in findings and the shortcomings of prior literature reviews. Section III describes the methods used to conduct our review, including strategies to cull the literature for studies comparing the effects of custodial and noncustodial sanctions, how effect sizes and methodological variations were coded, and statistical methods used to analyze the data. Section IV reports the results of the meta-analytic review. Compared with noncustodial sanctions, custodial sanctions have a null to slightly criminogenic effect on reoffending. The robustness of this overall effect is shown through moderator analyses that assess whether effect sizes vary by methodological, sanction-related, and other characteristics of the studies examined. Based on our findings and prior reviews, Section V concludes that the null effects of custodial sanction on reoffending should be considered a “criminological fact.” We conclude by exploring the implications of this finding for correctional policy in the United States.

I. Competing Perspectives on Imprisonment

In this section we examine the competing individual deterrence and criminalization perspectives on the effects of imprisonment on inmates' subsequent criminality. Theoretical and “common sense” arguments that the experience of imprisonment should reduce prisoners' subsequent offending have never been strongly supported by the specialist literatures.

A. Incarceration as a Specific Deterrent

The notion that offenders are rational, calculating individuals who can be deterred from future offending by increasing the severity of punishments gained widespread support among politicians and citizens between the 1970s and 1990s, particularly in the United States (Garland 2001; Clear and Frost 2014; Pratt 2019). The theoretical logic is that individuals engage in crime because it furthers their self-interest (Clarke and Cornish 2001; Chalfin and McCrary 2017). Faced with an opportunity to engage in criminal

activity, the offender can choose either to commit the crime and receive its benefits or not commit the crime and receive no benefit. Passing up the chance to commit a crime is both risk- and reward-free, while seizing the criminal opportunity carries both a reward and a risk of potential apprehension. Thus, the choice comes down to a cost-benefit analysis of the magnitude of the potential benefits of the act weighed against a combined function of the certainty of apprehension and the severity of the expected punishment (Becker 1968). Following this logic, the threat of incarceration provides some level of deterrence to the population at large (i.e., general deterrence), and the experience of incarceration for individual offenders is sufficiently aversive that the expected utility of future criminal involvement is overshadowed by its costs (i.e., specific deterrence). Furthermore, to the extent that deprivations imbued by incarceration are perceived as more severe than those of a noncustodial sanction, incarceration should exert a greater specific deterrent effect than sanctions such as probation (Nagin, Cullen, and Jonson 2009).

Despite the intuitive appeal of the notion that harsher punishments will deter criminal behavior, there are several logical and empirical reasons to be skeptical. Three are most salient. First, rates of reoffending for those sentenced to incarceration are high (Beck and Shipley 1989; Langan and Levin 2002; Alper, Durose, and Markman 2018). In an important analysis, Langan and Levin (2002) showed that 67.5 percent of prisoners are rearrested within three years of release. More recent data report a similar 68.5 percent rearrest rate of prisoners after three years and an 83 percent rate within nine years (Alper, Durose, and Markman 2018). Advocates of deterrence rarely discuss the failure rate of imprisonment in discouraging reoffending among released prisoners.

Second, Becker's (1968) influential model of deterrence posits that both certainty and severity of punishment are required for any particular sanction to deter behavior.¹ According to this model, the probable deterrent effect of a sanction that is severe yet highly unlikely to occur is limited (Nagin 2013; Chalfin and McCrary 2017). This is precisely the situation for

¹ Early writings by Beccaria and Bentham also stressed the importance of the celerity (i.e., swiftness) of the sanction. This is often overlooked. Beccaria ([1764] 1986, p. 36) noted, for example, that "the more promptly and the more closely punishment follows upon the commission of a crime, the more just and useful it will be." Available evidence on sanction celerity is not abundant and results are mixed, but it suggests that delaying punishment—even by just a little—erodes its potential deterrent effect regardless of how certain or severe it may be (for a review, see Pratt and Turanovic 2018).

incarceration as a source of deterrence: before an individual is sentenced to prison, the crime must be reported, the incident investigated, the correct offender apprehended, charges filed, the prosecution's arguments accepted by the judge or jury, and the sentence chosen must be a period of incarceration. As Durlauf and Nagin (2011) point out, "none of these successive stages in processing through the criminal justice system is certain" (p. 16). Stated another way, because the vast majority of criminal events do not result in imprisonment (Sellin 1931; Coleman and Moynihan 1996; Mosher, Miethe, and Phillips 2002), the deterrent effect of possible time behind bars relies primarily upon severity. This reliance on the threat of a severe yet unlikely term of imprisonment is problematic; existing reviews of the literature suggest preventive strategies focused on increasing the certainty of punishment are much more effective (e.g., problem-oriented policing such as Operation Ceasefire; see Durlauf and Nagin 2011; Chalfin and McCrary 2017; Braga, Weisburd, and Turchan 2018).

Although similar arguments about the uncertainty of noncustodial alternatives such as probation or community-based treatment could be made, the data are clear that probation is a far more likely outcome of a conviction than incarceration (National Academy of Sciences 2014). To be sure, rates of reoffending for offenders sentenced to probation are also high, with between 30 and 40 percent rearrested within three years (Petersilia 2002; Texas Legislative Board 2019). However, the groups of offenders sentenced to prison versus probation are likely to be quite different in terms of criminal history, offense seriousness, and other characteristics. Adequate tests of the efficacy of custodial sanctions in reducing reoffending need to take account of factors that influence selection into the "treatment" of receiving a custodial sentence.

Third, the deterrent value of custodial sanctions assumes that offenders perceive such sanctions as being more severe than noncustodial sanctions; however, existing research suggests this is not always the case. Many offenders would rather serve shorter prison terms (e.g., 1 year) than lengthier community-based punishments with intensive conditions (Petersilia 1990; Crouch 1993; Moore, May, and Wood 2008). Furthermore, individual differences among offenders and specific aspects of sentences appear to influence the perceived severity of incarceration. Work by Raaijmakers et al. (2017) and Crank and Brezina (2013) indicates that inmates with a greater level of commitment to a criminal lifestyle (e.g., "Committing crime is pretty much a permanent way of life") perceive prison time as being less difficult than those with low levels of commitment. Raaijmakers et al. (2017)

also find that the subjectively experienced severity of incarceration decreases over the course of incarceration. The impact of custody might lose its sting for inmates strongly committed to criminality or who simply grow accustomed over time to spending time inside prison walls.

B. Incarceration as a Criminogenic Experience

Whereas deterrence theorists reduce incarceration to a price tag that affects calculations about crime's costs and benefits, other scholars—especially sociologists—see time in prison as a lengthy social experience that can have criminogenic effects. Cullen, Jonson, and Nagin (2011, p. 535) observe, for example, that inmates regularly “associate with other offenders, endure the pains of imprisonment, risk physical victimization, are cut off from family and prosocial contacts on the outside, and face stigmatization.” Thus, although prisons are intended to deter future offending, this perspective holds that incarceration exposes individuals to criminogenic risk factors and distances them from protective factors, thus increasing the prospect of reoffending upon release.

First, as places where offenders live together in a “society of captives” (Sykes 1958), the prison has long been referred to as a “school of crime” (Bentham [1789] 1970) or “house of corruption” (Shaw 1930) because of the likelihood that techniques of and motivations for crime are transmitted between inmates. The principles of social learning theory, when applied to the prison context (Sutherland 1939; Akers 2009), suggest that as inmates are exposed to a large group of antisocial peers and cut off from any prosocial peers on the outside, they increasingly come into contact with pro-criminal attitudes (e.g., the “convict code”; see Irwin and Cressey 1962; Mears et al. 2013); learn from and imitate others' behavior in order to adjust to prison life and offending in general; and receive social and tangible reinforcements for adhering to prison culture. Several studies indicate an important role for peer effects in prisoners' postrelease offending (e.g., Bayer, Hjalmarsson, and Pozen 2009; Ouss 2011; Damm and Gorinas 2016). In their examination of peer effects among juvenile offenders, Bayer and colleagues (2009) find that an inmate's exposure to a greater proportion of inmates with the same conviction offense increases reoffending within that particular crime category. To illustrate, for an individual committed for aggravated assault, increases in the proportion of fellow inmates convicted of aggravated assault were associated with higher odds of the focal inmate committing another assault upon release. Subsequent work examining cell-level interactions

offers similar conclusions, and additionally shows that peer effects are more evident for skill-intensive offenses (e.g., theft, drug-dealing) than for violent crimes (Ouss 2011; Damm and Gorinas 2016; cf. Harris, Nakamura, and Bucklen 2018).

Second, inmates are often exposed to the types of adverse events hypothesized to increase psychological strain and criminal coping. Agnew's (1992) general strain theory (GST) posits that failure to achieve positively valued goals, removal of positive stimuli, and introduction of negative stimuli often lead to criminal behavior because they cause negative emotional states (e.g., anger, depression) and pressure to cope with those states. The personal and social resources of the individual, such as self-efficacy, self-control, socioeconomic status, and social control, moderate the association between the negative emotions experienced and the coping strategy chosen—whether prosocial or antisocial (Agnew 2001, 2013; Thaxton and Agnew 2018; Hoffman 2019). Blevins and colleagues (2010) argue that these propositions are highly relevant to common experiences within prison walls. For example, positively valued goals that may be blocked during incarceration include failure to obtain work assignments or having visitation privileges revoked; positive stimuli that are removed include a sense of autonomy, personal identity, heterosexual relationships, and privacy, among others (Sykes 1958; Toch 1977; Crewe 2011); and prison crowding, increased victimization, exposure to violence, and the requirements of psychological assessments and rehabilitation programs represent the introduction of negative stimuli (Wolff et al. 2007; Crewe 2011; Zweig et al. 2015). Furthermore, it is well established that many inmates possess pre-prison or confinement-induced characteristics that potentially reduce the likelihood of prosocial coping, including low self-control (Hochstetler and DeLisi 2005), a commitment to the “code of the streets” (Mears et al. 2013), and infrequent contact with the outside world (Cochran et al. 2015).

To date, only a handful of studies have directly tested the applicability of GST in predicting the antisocial outcomes of inmates (Morris et al. 2012; Listwan et al. 2013; Zweig et al. 2015). Listwan et al. (2013) collected data on prisoners recently released from incarceration to test how in-prison experiences related to rearrest and recommittal over a 2.5-year follow-up period. Their analyses indicated that negative prison environments (e.g., inmates were more afraid of being assaulted, threatened) were associated with increased odds of both rearrest and recommittal, and direct victimization by other inmates was associated with higher odds of recommittal. Zweig et al. (2015) similarly examined the effects of in-prison victimization on

self-reported offending over a 15-month follow-up using data from the Serious and Violent Offender Reentry Initiative. In accordance with GST, they showed that victimization while incarcerated had both direct and indirect (through hostility) effects on measures of any crime and violent crime after release, and both direct and indirect (through depressive symptoms) effects on drug use.

Third, incarceration has been hypothesized to increase reoffending through the labeling effect of a criminal record and associated collateral consequences. The classic labeling theory argument is that stigmatizing societal reactions are internalized and that this deviant identity ensnares individuals in a criminal trajectory (Lemert 1951; Chambliss 1973). A related hypothesis is that having a conviction or imprisonment record stabilizes offending by weakening social bonds with family and limiting opportunities for employment, education, and housing (Uggen and Stewart 2015; Kirk and Wakefield 2018).

The strongest evidence for labeling theory is found in studies that prospectively compare the reoffending outcomes of individuals who have had contact with the criminal justice system against the outcomes of those who avoid system contact. These studies tend to find that arrests, court appearances, convictions, and other forms of system contact are associated with an increased risk of subsequent involvement in criminal behavior (e.g., Bernburg, Krohn, and Rivera 2006; Chiricos et al. 2007; Lopes et al. 2012; Petitclerc et al. 2013; Petrosino, Turpin-Petrosino, and Guckeberg 2013; Liberman, Kirk, and Kim 2014; Wiley and Esbensen 2016; Motz et al. 2020). For example, Wiley and Esbensen (2016; see also Wiley, Slocum, and Esbensen 2013) used propensity score techniques to examine the effects of police contact among participants in the Gang Resistance Education and Training program. Matching on a range of relevant confounders (e.g., demographics, risk-seeking, prior delinquency, peer delinquency), Wiley and Esbensen showed that individuals who were stopped by police or arrested subsequently engaged in more frequent delinquency than those who had no police contact. Participants who were arrested also engaged in more subsequent delinquent behavior than those who were only stopped and questioned. Liberman, Kirk, and Kim (2014) also used propensity score matching (PSM) to investigate the influence of arrests during adolescence on subsequent delinquency for participants surveyed in the Project on Human Development in Chicago Neighborhoods. With propensity scores based on a robust set of 79 covariates, Liberman and colleagues found that being arrested was associated with increases in the subsequent variety of offending and higher odds of rearrest.

Another recent study conducted by Motz et al. (2020) capitalized on data from both fraternal and identical twins collected during the Environmental Risk Longitudinal Twin Study. After adjusting for a range of relevant covariates and genetic confounding, Motz et al. found that being arrested, having an antisocial behavior order issued by the court, and receiving an official criminal record each increased the likelihood of subsequent involvement in delinquency.

Prior research thus confirms labeling theory's hypothesis that criminal justice system contact increases criminal behavior. Evidence for the intervening mechanisms set forth by the theory is less clear. To date, there have been few empirical assessments of whether arrest, conviction, or imprisonment actually cause the changes in offenders' identities that are the focal point of labeling theory. The handful of studies have produced mixed findings on whether formal intervention leads to the adoption of a criminal identity (Jensen 1972; Ageton and Elliott 1974; Hepburn 1977; Brownfield and Thompson 2008; Wiley, Slocum, and Esbensen 2013). These studies are also limited by the use of cross-sectional data that cannot account for temporal ordering and/or inadequate measurement of identity. For example, Hepburn (1977) analyzed cross-sectional data from police records and surveys with a sample of 145 adolescent males to determine the effect of formal labeling on identity. Delinquent identity was captured by a single survey item asking participants how often they thought of themselves as delinquent. Hepburn (1977) found no significant association between contact with the police and delinquent identification after accounting for socioeconomic status and self-reported delinquency. More recently, Wiley, Slocum, and Esbensen (2013) used longitudinal data from middle-schoolers involved in the Gang Resistance Education and Training study to evaluate the labeling effects of police contact. Deviant identity was not directly measured. Rather, the authors used items tapping attitudes (e.g., degree of guilt about attacking something with a weapon) and beliefs (e.g., "It's okay to steal something if that's the only way you could ever get it") about delinquency, which they argue precede the adoption of a deviant identity. After matching participants using propensity score techniques, Wiley and colleagues found that individuals who were arrested subsequently had more crime-supportive attitudes and beliefs about crime than individuals who had no contact or were only stopped by police.

Numerous studies have examined whether official labeling has negative effects on other intermediate outcomes such as social and economic opportunities. In examining employment, for example, experimental data

consistently indicate that employers are significantly less likely to call back or hire individuals who have criminal records than those without (e.g., Pager 2003; Decker et al. 2015; DeWitt and Denver 2020). Pager (2003) found that, among otherwise identical candidates, individuals with a criminal record are less likely to receive callbacks from employers than those without a record. This effect was further moderated by race: White candidates with a criminal record were more likely to receive a job callback than Black candidates without a record. Available evidence from surveys and interviews with employers suggests their hesitancy about hiring ex-offenders stems from concerns over financial or reputational damage and worker unreliability (Holzer, Raphael, and Stoll 2002; Fahey, Roberts, and Engel 2006; Goodstein and Petrich 2019).

The results from studies isolating the effects of incarceration on individual offenders' postrelease employment tend to be mixed, with some showing that employment outcomes are worse following incarceration (e.g., Sampson and Laub 1993; Western and Pettit 2005; Apel and Sweeten 2010) and others finding null effects of incarceration on employment (e.g., Kling 2006; Loeffler 2013; Verbruggen 2016). In general, though, studies finding null effects tend to be more recent and made use of more sophisticated analytical techniques (e.g., natural experiments, PSM) to account for confounder imbalances between incarcerated and nonincarcerated groups. Loeffler (2013; see also Kling 2006), for example, used an instrumental variable approach to analyze data on over 20,000 offenders in Cook County, Illinois. After instrumenting on random judge assignment, he showed that being sentenced to incarceration has no discernible effect on employment over a five-year follow-up period. In their analyses of the National Longitudinal Survey of Youth, 1997, Apel and Sweeten (2010) use both fixed effects regression models and PSM to examine the impact of incarceration on employment. With both analytical techniques, they find that incarceration does lead to unemployment. However, their data also show that this unemployment was due to formerly incarcerated participants not actively looking for work as opposed to difficulties in obtaining a job, *per se*. These findings suggest that although labor market opportunities may be available, former prisoners may lack the motivation to seek them out (Visher, Debus-Sherrill, and Yahner 2011).

To summarize, proponents of the mass incarceration movement claim that the experience of imprisonment is so aversive that it deters offenders from engaging in future offending. There are several reasons, however, to expect that any deterrent effect is overshadowed by the criminogenic

features of imprisonment and its collateral consequences. Prisoners are surrounded by other prisoners, and they can engage in a mutual exchange of knowledge and motivation for criminal behavior. They also risk being victimized by other prisoners and feel the strains of being cut off from family, employment, and other ties to conventional society. The mark of a criminal record is a major hurdle to finding employment and, accordingly, to securing housing, providing for their own and their families' basic needs, and permanently desisting from crime. Thus, ample grounds exist to expect that imprisoning offenders has limited or iatrogenic effects on reoffending.

II. The State of Research on Custodial Sanctions and Reoffending

Prior research examining the effects of custodial sanctions has produced discrepant findings. Some studies find that terms of incarceration reduce reoffending when compared to noncustodial sanctions such as probation, community service, and other community punishments (e.g., Jones and Ross 1997*b*; Hjalmarsson 2009; Bucklen 2014; Bhuller et al. 2016). By contrast, a larger body of research reports that custodial sanctions either have no effect (e.g., Loeffler 2013; Harding et al. 2017; Mitchell et al. 2017) or increase reoffending (e.g., Aizer and Doyle 2015; Gilman, Hill, and Hawkins 2015; Mears and Cochran 2018). Conducting a meta-analytic review is a means of assessing what effect these studies reveal when taken together as a whole. Prior to presenting the systematic analysis, we discuss several likely reasons for the heterogeneity in effect estimates in prior research, including variations in methodological characteristics, the types and lengths of sanctions, and other aspects of studies such as the age and gender distributions of their samples. We conclude by considering the results of prior systematic reviews of the literature on custodial sanctions, the shortcomings of those reviews, and advances in the research over the past decade that motivated this meta-analytic review.

A. Sources of Heterogeneity in Findings

There are three prominent, potential sources of the heterogeneity in findings on the effects of custodial sanctions. Given their theoretical salience, these factors were included in moderator analyses in our meta-analysis. First, large discrepancies exist in the methodological quality of studies examining the influence of custodial sanctions on reoffending.

Although the randomized controlled trial (RCT) is considered the gold standard for reaching estimates of the causal effect of interventions (Shadish, Cook, and Campbell 2002), the practical and ethical realities of randomly assigning offenders to prison versus probation make RCT-based evaluations rare. Only five such studies have been conducted to date (Bergman 1976; Schneider 1986; Barton and Butts 1990; Killias, Aebi, and Ribeaud 2000; Killias et al. 2010). Among them, the Killias et al. (2000, 2010) studies rely on the same data and, together with the Schneider (1986) study, examine the effect of two weeks or less of incarceration. The Barton and Butts (1990) RCT examines a longer period of incarceration (mean = 12.8 months), while the amount of time is unspecified in Bergman (1976); both use data that are over 30 years old.

In light of the difficulties with conducting RCTs, the vast majority of prior research has used observational data coupled with varying levels of analytical rigor to determine the effects of custodial sanctions. At one end of the spectrum are analyses that employ neither matching procedures nor statistical controls to account for nonrandom treatment assignment. This limitation often characterizes reports from state and federal departments of correction. For example, McAlister, Officer, and Sanchagrin (2019) reported the three-year recidivism rates for biannual cohorts of offenders released from prison or placed on probation in Oregon between 1998 and 2016. Not surprisingly, the majority of effect size estimates from this and similar reports point to a large criminogenic effect of prison, likely because people sentenced to prison tend to differ substantially from people sentenced to probation. For example, compared with offenders sentenced to noncustodial sanctions, those sentenced to custody tend to have longer criminal records and more serious conviction offenses, thus signaling a greater risk for reoffending (Sweeten and Apel 2007; Wermink et al. 2010; Aizer and Doyle 2015).

Other studies improve estimates by using regression-based techniques to control for relevant covariates or quasi-experimental methods such as exact matching, PSM, or instrumental variable analysis. Nagin, Cullen, and Jonson (2009; see also Bales and Piquero 2012*a*; Gaes, Bales, and Scaggs 2016) have previously reviewed the potential problems associated with regression-based analyses of prison effects (e.g., imprecision of specifying age effects, linearity assumption, over-parameterization), urging researchers to opt instead for quasi-experimental designs. Even among these methodologies, however, there is variability in model specification that can be consequential for effect size estimates. For example, Bales and Piquero

(2012*a*) show that the sizes of estimates from both exact matching and PSM are sensitive to the theoretical constructs that matching procedures account for. Similarly, Gaes, Bales, and Scaggs (2016) illustrate that, despite matching on the same set of covariates, radius matching, coarsened exact matching, and exact matching techniques yield substantially different estimates in terms of size, directionality, and statistical significance.

Beyond these differences in research design and model specification, the primary studies included in our analysis differ on several other methodological characteristics that might affect estimates of the effects of custody on reoffending. The size of the samples used in individual statistical models ranged from less than 100 (e.g., Wheeler and Hissong 1988*a*; Steiner and Giacomazzi 2007; Sirén and Savolainen 2013) to more than 500,000 (e.g., Mueller-Smith 2014; Gaes et al. 2016). This variation in sample size influences the confidence accorded the effect sizes reported by individual studies, with smaller samples being more prone to imprecision (Dattalo 2008; Barnes et al. 2020). Primary studies likewise differ in how reoffending is measured. Depending on the data available to researchers, reoffending can be captured by indicators of rearrests, reincarceration, violations of community supervision orders, or new charges or convictions. The period of time during which these forms of reoffending are measured also varies between studies, ranging from as little as six months (e.g., Scarpitti and Stephenson 1968) to as much as 15 years (e.g., Gilman, Hill, and Hawkins 2015).

Second, there is also considerable heterogeneity in characteristics of the custodial and noncustodial sanctions assessed (Nagin, Cullen, and Jonson 2009; Villettaz, Gillieron, and Killias 2015). One of the central arguments made by proponents of imprisonment is that sanctions that are more severe will be more likely to deter reoffending (Becker 1968; Wilson 1975). Thus, to the extent that the conditions and lengths of custodial sanctions differ, estimates of the effectiveness of incarceration may vary as well. Deterrence theorists would predict, for example, that the custodial deterrent effect of prison would likely be larger than that of shock incarceration, and that longer sentences would yield larger deterrent effects than shorter ones. The studies included in our sample examine a range of custodial sanctions, including prison, jail, juvenile detention, residential treatment, boot camp, and shock incarceration. On the noncustodial side, comparison groups were sentenced to regular probation, intensive probation, electronic monitoring, community service or fines, fully suspended sentences, dismissals, and nonresidential treatment.

Beyond the types of sanctions examined, there is also variability in the literature in the specific combinations of sentences that are examined and the durations of custody. For example, both Gilman, Hill, and Hawkins (2015) and Wermink et al. (2010) examine the impact of short-term incarceration (i.e., less than 60 days) on reoffending. However, Gilman et al. do so with a sample of juveniles compared with others who were arrested but not incarcerated, whereas Wermink et al. compare short-term custody with community service within a sample of adults. Some analysts working with large data sets collapse multiple types of sanctions into singular “custodial” and “noncustodial” categories. In their analyses of 330,971 offenders in Florida, for example, Mitchell et al. (2017) combined those who had served time in prison with jail inmates and those who had served regular probation with those on intensive probation. Using a larger data set from Florida, Mears and Cochran (2018) chose to disaggregate these groupings for increased specificity in effect estimates. Most of the existing research on the effects of custody does not report the mean length of time participants served behind bars. However, among those that do, there is also considerable variation. The majority examine custodial sanctions of between one and six months (e.g., Killias et al. 2000; Wodahl, Boman, and Garland 2015), but some consider sentences of less than one month, between six months and a year (e.g., Abrams 2010; Freiburger and Iannacchione 2011; Robert et al. 2017), or two years or more (e.g., Harding et al. 2017).

Third, samples differ on a range of sociodemographic factors that allow researchers to test the generality of the effects of custody. Deterrence theory is general in its prescriptions about punishment and criminal behavior: custody should exert more of a deterrent effect than noncustodial sanctions regardless of the age and gender of the offender and the country and the time of incarceration. We included studies produced between 1965 and 2019 and that used data collected in 15 different countries. Within this group of studies, there is much diversity in the age and gender distributions of samples. For example, most prior research is based upon mixed-gender samples, and analysts generally do not conduct supplemental analyses to determine whether incarceration works differently for males and females (e.g., Wermink et al. 2010; Bales and Piquero 2012*a*; Mears and Cochran 2018). However, some researchers investigated sanction effects exclusively among males (e.g., Cochran, Mears, and Bales 2014; Jolliffe and Hedderman 2015), exclusively among females (e.g., Hedderman and Jolliffe 2015), or in analyses stratified by gender (e.g., Mitchell et al. 2017; Caudy, Skubak Tillyer, and Tillyer 2018). Another source of heterogeneity is the age

composition of samples. The majority of research uses samples composed solely of adults (e.g., Jones and Ross 1997*a*, 1997*b*; Mears, Cochran, and Bales 2012; Harding et al. 2017). However, 25 studies included in our analysis focus exclusively on juvenile offenders (e.g., Sweeten and Apel 2007; Aizer and Doyle 2015; Gilman, Hill, and Hawkins 2015).

Although deterrence theory suggests general effects of custody on reoffending when compared with noncustodial sanctions, other theory and research point to important differences between individuals and contexts that might lead to heterogeneity in the effectiveness of custody. With regard to gender differences, for example, scholars have argued that women tend to struggle more than men with adapting to prison life because of higher rates of mental health and drug use problems, histories of abuse and trauma, and the strains of family separation (Slotboom et al. 2011; Mahmood et al. 2012; Kruttschnitt et al. 2013). These gendered differences could conceivably lead to divergent effects of custody on reoffending. Likewise, because of the neural, psychological, and social plasticity inherent to adolescence (Laub and Sampson 2003; Somerville, Jones, and Casey 2010; Sullivan 2020), sentencing juveniles to imprisonment may have a greater effect—for better or for worse—than for adults. The empirical issue remains as to whether sample characteristics such as gender and age moderate sanctioning effects.

B. Prior Reviews of the Literature

Considering the variability in characteristics of the samples, sanctions, and designs of prior research, individual studies are limited in their ability to reach a meaningful conclusion about the efficacy of custodial sanctions in reducing reoffending. Accordingly, several groups of scholars have attempted to make sense of this literature through meta-analytic and narrative reviews. Five such reviews have been conducted, each concluding that custodial sanctions have either a null or criminogenic effect on reoffending.

First, in a report to the Solicitor General of Canada, Smith, Goggin, and Gendreau (2002) meta-analyzed the effects of custodial sanctions by drawing on 104 effect sizes garnered from 31 studies. The mean phi coefficients in their analyses were between .07 and .00 (unweighted and weighted by sample size, respectively), indicating a small criminogenic or null effect of custodial sanctions on reoffending. Smith and colleagues examined only

a small number of potential moderators. However, these analyses indicated that criminogenic effects were larger in studies of juveniles than adults (weighted $\phi = .08$ vs. $.03$) and in studies of strong compared to weaker methodological quality (weighted $\phi = .08$ vs. $-.01$). The modeling strategy used did not explicitly account for the statistical dependence of multiple effect sizes drawn from some individual studies. Nonetheless, sensitivity analyses showed that the mean effect size estimate remained the same when multiple, overlapping effect sizes from individual studies were excluded.

Second, Nagin, Cullen, and Jonson (2009) conducted a systematic narrative review of the literature on custodial sanctions and reoffending that was published in *Crime and Justice*. They chose not to meta-analyze the available research on custodial sanctions because of the between-study heterogeneity in sample characteristics, sanctions being examined, and the quality of those studies. Results of 55 studies were examined, including five RCTs, 12 matching studies, and 31 regression-based studies; the remaining seven were an assortment of natural experiments, inverse probability of treatment-weighted analyses, and other designs. The main conclusion was that incarceration seems to have a null or criminogenic effect on subsequent offending. However, Nagin, Cullen, and Jonson (2009) pointed out that much of the research then available was methodologically inadequate. Studies that properly accounted for selection into sentences of imprisonment, such as natural experiments and propensity-score-based analyses, were in short supply. Accordingly, they urged substantial improvements in methodological rigor, noting that “as studies on the impact of imprisonment on reoffending become more plentiful and of a higher quality, the application of meta-analysis to the extant body of evidence would be useful” (Nagin, Cullen, and Jonson 2009, p. 143).

Third, Jonson (2010) meta-analyzed 177 effect size estimates from 57 studies, finding a small criminogenic effect of custodial sanctions (mean $r = .144$). In contrast to Smith, Goggin, and Gendreau (2002), Jonson’s (2010) moderation analyses showed that effect sizes were larger in samples comprised exclusively of adults and that methodological quality was not a significant moderator. Furthermore, effect size estimates varied by the types of both custodial and noncustodial sanctions served and the gender composition of the samples examined. For example, studies that examined prison or shock probation (e.g., six-month prison sentence, plus probation) as the custodial sanction found larger criminogenic effects of custody than studies that examined jail, juvenile detention, and boot camp.

Although the larger number of effect sizes and methodological moderators Jonson examined constituted an improvement over Smith and colleagues' (2002) work, the modeling technique she used did not take into account the statistical dependence of effect sizes, nor were sensitivity analyses conducted to examine the influence of such dependence on mean effect size estimates.

Fourth, in the most recent meta-analytic review, Villettaz, Gillieron, and Killias (2015) provided an update to an earlier Campbell Review (Villettaz, Killias, and Zoder 2006) by examining sanction effects among 14 studies. Focusing solely on the results from RCTs and natural experiments ($k = 5$), custodial sanctions had a null effect on reconviction relative to noncustodial sanctions (mean odds-ratio [OR] = .946, $p > .05$). Among quasi-experimental studies ($k = 9$), a small but criminogenic effect of custodial sanctions was observed (mean OR = .684, $p < .001$). A limitation of this work is that Villettaz and colleagues (2015, p. 42) made the choice to include only a single effect size from each study, noting that "given the fact that the overall results favoured the null hypothesis, the strongest effect sizes have been used as a conservative way to minimize the chance of obtaining a non-significant outcome." Because of the restrictive inclusion criteria adopted by Villettaz, Gillieron, and Killias (2015), the selection of one effect size per study, and the small number of studies, no moderator analyses were conducted.

Fifth, Loeffler and Nagin (2021) reviewed a subset of studies investigating the effect of incarceration on reoffending. In addition to studies comparing custodial to noncustodial sanctions, they included examinations of the effects of pretrial detention and the length of custodial sanctions. Their narrative review focused solely on the results of natural experiments—namely, those that use random judge assignment for instrumental variable analysis or regression discontinuity designs to capitalize on discontinuities created by sentencing guidelines. These natural experiment designs are powerful in their ability to approximate the random and even distribution of confounders that is the hallmark feature of RCTs. Considering the findings from 19 such studies, Loeffler and Nagin (2021) conclude that, "with only two exceptions, . . . post-conviction imprisonment has no effect on reoffending or exacerbates it." Given the similarity of their conclusion with those of prior reviews, they also note that concerns about lack of control for unobserved characteristics of offenders in standard regression and matching studies were likely exaggerated (see, e.g., Nagin, Cullen, and Jonson 2009; Aizer and Doyle 2015; Villettaz, Gillieron, and Killias 2015; Mitchell et al. 2017).

C. The Current Review

Against the theoretical, empirical, and methodological backdrop laid out thus far, our aim was to conduct an updated meta-analytic review of research comparing the effects of custodial and noncustodial sanctions on reoffending. We expand upon prior work in three important ways. First, this is the first comprehensive meta-analytic review of the literature in over a decade (i.e., since Jonson 2010). Although the review by Villettaz, Gillieron, and Killias (2015) was published six years ago, their analyses were limited by a restrictive set of inclusion criteria and by the selection of only one effect size from each of the 14 studies included (e.g., one from among 36 effect sizes reported in Bales and Piquero 2012*a*). In addition to including 22 quasi-experimental studies published since Villettaz and colleagues (2015), we relaxed the inclusion criteria to allow regression-based, exact matching, and unadjusted comparisons of individuals sentenced to custodial and noncustodial sanctions. Doing this allowed us to take stock of the entire body of research, comprising 981 effect size estimates drawn from 116 studies. Of these, approximately two-thirds (655) were drawn from 55 studies released since 2010.

Second, in contrast to past reviews, the large sample of effect size estimates and detailed coding of studies enabled us to examine whether variations in research methods, sanctions, and sociodemographic characteristics (e.g., age and gender distributions, country) moderate effect size estimates. Including studies with wide variability in methodological rigor increases the heterogeneity of effect sizes. Some scholars suggest that analyzing studies with such differences is akin to comparing apples and oranges (e.g., Eysenck 1984; Sharpe 1997), and that their inclusion in a meta-analysis may bias mean effect size estimates. These critics favor conducting meta-analyses on a relatively homogenous—which typically means small—set of studies with only the highest methodological rigor. Other prominent meta-analysts, however, contend that all studies on a topic should be included (e.g., Smith, Glass, and Miller 1980; Glass 2015; Turanovic and Pratt 2021). As Glass (2015; see also Greenland 1994) notes, “all studies differ, and the interesting questions to ask about them concern how their results vary across the factors we conceive of as important” (p. 225).²

² Glass (2015) also comments that he is “staunchly committed to the idea that meta-analyses must deal with all studies—with good or bad and indifferent studies—and that their results are only properly understood in the context of each other, not after having been censored by some a priori set of prejudices” (p. 229).

Restricting a review to a small subset of studies eliminates the possibility of discerning which moderators affect the size and direction of the effect sizes.

Treating the differences between studies as an empirical matter to be investigated has important implications for both theory and methodological choices. At the theoretical level, it is worth exploring, for example, whether the deterrence hypothesis finds support only in studies with very low methodological quality. Examining variations in effect size estimates across a range of methodological moderators can also provide guidance for future research aiming to produce more reliable estimates of sanction effects. To these ends, we explicitly coded for a large number of characteristics such as within- and between-study differences in the overall research designs employed (e.g., natural experiment, RCT, PSM, regression-based), variables controlled for or matched on (e.g., age, sentence length, prior record), the types of sanctions served (e.g., jail, prison, probation, intensive probation), and sample demographics (e.g., gender and age composition). Doing so with a large, comprehensive sample of primary studies and effect size estimates means that heterogeneity is an advantage, allowing for the explicit identification of factors that do or do not moderate effect sizes.

Third, we capitalize on improvements made in meta-analytic modeling techniques since the studies by Smith, Goggin, and Gendreau (2002) and Jonson (2010)—namely, multilevel modeling (MLM; see Borenstein et al. 2009; Hox, Moerbeek, and van der Schoot 2018). One advantage of the MLM approach is that greater weight is assigned to effect size estimates that are more reliable by explicitly accounting for their standard errors (discussed further in Section III). This approach results in greater precision when estimating mean effect sizes than earlier techniques, such as weighting effect sizes solely by sample size (i.e., the practice used in both Smith, Goggin, and Gendreau [2002] and Jonson [2010]; see Pratt et al. [2014] for further elaboration). The MLM approach also allows for the inclusion of multiple effect sizes from each study by accounting for the dependence of observations. Prior meta-analytic reviews conducted without MLM have not accounted for such dependence (Smith, Goggin, and Gendreau 2002; Jonson 2010). Furthermore, although Villettaz, Gillieron, and Killias (2015) used MLM, they chose to include a single effect size from each study in their models. Both the failure to account for the dependence of observations within studies and the researchers' decisions to include specific effect sizes can bias meta-analytic results (Becker 2000; Pratt and Cullen 2000). The approach we use alleviates both concerns, providing

the most comprehensive estimate to date of the effect of custodial sanctions on reoffending.

III. Methods for Assessing Custodial Sanctioning Studies

In this section, we outline the methods used to conduct our meta-analysis. We describe the multiple techniques that were used to cull the literature, the criteria for including studies, and how effect size estimates were calculated from statistical models within each study. We also discuss the methodological, sanction, and sample characteristics that were coded for each individual effect size estimate and used to test for the possibility of effect moderation. We conclude with an explanation of the MLM framework that was used to meta-analyze the sample of effect size estimates, arrive at mean effect sizes, and conduct moderation analyses.

A. Sample

Our sample is composed of studies produced through May 2019. They were systematically gathered in four ways. First, all prior issues of top-ranking criminology and criminal justice journals were reviewed for studies that compared the reoffending outcomes of groups sentenced to custodial versus noncustodial sanctions.³ Second, extensive searches of electronic databases (i.e., ProQuest Criminal Justice [including dissertations]; EBSCO Criminal Justice Abstracts with Full Text; PsycINFO [including dissertations]; Google Scholar; and JSTOR Economics) were conducted with combinations of search terms that captured both the sanction type (i.e., “prison,” “imprison*,” “incarcerat*,” “boot camp,” and “custodial”) and outcomes of interest (i.e., “recidivism,” “reoffend*,” “rearrest,” “reincarcerate*,” and “reconvict*”). Third, the reference lists of studies located through the first two steps and of prior reviews (i.e., Smith, Goggin, and Gendreau 2002; Nagin, Cullen, and Jonson 2009; Jonson 2010; Villettaz, Gillieron, and

³ These journals included: *Australian and New Zealand Journal of Criminology*, *British Journal of Criminology*, *Crime & Delinquency*, *Criminal Justice and Behavior*, *Criminology*, *Criminology & Criminal Justice*, *Criminology & Public Policy*, *European Journal of Criminology*, *International Journal of Offender Therapy and Comparative Criminology*, *Journal of Contemporary Criminal Justice*, *Journal of Criminal Justice*, *Journal of Developmental and Life-Course Criminology*, *Journal of Experimental Criminology*, *Journal of Offender Rehabilitation*, *Journal of Quantitative Criminology*, *Journal of Research in Crime and Delinquency*, *Justice Quarterly*, *Nordic Journal of Criminology*, *Punishment and Society*, *Prison Journal*, and *Youth Violence and Juvenile Justice*.

Killias 2015) were examined for studies not already captured. Fourth, state and federal correctional agencies' websites were searched for unpublished comparisons of postsanction outcomes for custodial and noncustodial groups.

The satisfaction of three criteria was required for a study's inclusion in the meta-analysis. First, the study must have included a group of offenders sentenced to time in a custodial setting and a comparison group given an alternative, noncustodial sanction. Second, the study must have included some measure of postsanction criminal behavior (e.g., rearrest, technical violation). Third, the study must also have included sufficient information to calculate the common effect size used in our analyses. These inclusion criteria are quite broad, although intentionally so; as described in further detail below, this allowed us to code for between- and within-study variations in a variety of sample, sanction, and methodological characteristics that potentially moderate effect sizes.

The literature search and subsequent evaluation of whether the collected studies met the inclusion criteria produced the analytic sample. It consists of 981 effect sizes calculated from 116 studies that represent approximately 4.5 million individual offenders in 15 different countries.⁴ The number of effect sizes exceeds the number of studies because the majority of studies included more than one statistical model from which an effect size could be calculated. As an example, Bales and Piquero's (2012a) comparison of groups sentenced to prison and intensive probation measured new felony convictions at one, two, and three years; included models that used six increasingly stringent groups of statistical controls; and modeled the effects of sanctions on reconviction, net of controls, using logistic regression, precision matching, and PSM. Taken together, the Bales and Piquero study alone thus yielded 36 effect size estimates.

Including multiple effect sizes from individual studies was done for two reasons. First, as pointed out elsewhere (e.g., Becker 2000; Pratt and Cullen 2000; Weisz et al. 2017), choosing a single effect size or averaging across effect sizes within a study can introduce researcher (e.g., picking the effect size showing the greatest criminogenic effect of custodial sanctions) or statistical bias (e.g., artificially reducing between-effect variance). Second, doing so also results in the loss of valuable within- and between-study

⁴ This figure is approximate given that some studies report the outcomes of multiple release cohorts that may include some of the same individuals.

information that can be used to explore the characteristics of studies that moderate effect sizes. Although including multiple effect sizes per study raises concerns over violating the assumption of statistical independence, as described in detail below, the multilevel modeling approach used in the current study adjusted for such dependence.

B. Effect Size Estimate

The effect size estimate in the current study represents the magnitude of the association between receiving a custodial (as compared to a non-custodial) sanction and subsequent reoffending. Effect sizes were calculated using the standardized correlation coefficient (i.e., r). The r coefficient was chosen because its interpretation is generally more intuitive than other test statistics typically employed as meta-analytic effect size estimates (e.g., Cohen's d), and other test statistics are easily converted into r coefficients. Specifically, t -ratios from linear models (e.g., ordinary least squares) were converted using $r = t / \sqrt{t^2 + n - 2}$, while z -ratios from nonlinear models (e.g., logistic and Cox regression) were converted using $r = z / \sqrt{z^2 + n}$ (Rosenthal 1994; Vartanian, Schwartz, and Bronwell 2007). Odds ratios were converted by taking their natural logarithm, converting to Cohen's d , and then converting to r (Borenstein et al. 2009).

Given the coding of sanction group membership (i.e., 0 = noncustodial; 1 = custodial), positive r coefficients signify that being sentenced to a custodial sanction is associated with an increased likelihood of reoffending relative to people sentenced to a noncustodial sanction. Negative r coefficients signify that a custodial sentence is associated with reductions in reoffending. For analytical purposes, effect size estimates were converted to $z(r)$ scores using Fisher's r to z transformation (i.e., $z = 0.5 * \ln(1 + r/1 - r)$; see Borenstein et al. 2009). As Pratt et al. (2014; see also Blalock 1972) note, the sampling distribution of the $z(r)$ score is assumed to approach normality, while the distribution of r is not. For the multilevel linear modeling approach that was used, a normal distribution of effect size estimates is required for unbiased tests of statistical significance, accurate mean effect size estimates, and tests of their moderation.

We recognize concerns over the potential drawbacks of effect sizes drawn from both bivariate and multivariate models. The main concern with bivariate models stems from failing to account for confounds, while in multivariate models, study-to-study differences in the confounds that are accounted for may be large (see discussion in Pratt et al. 2014). These

issues were addressed in two ways. First, we coded for whether effect sizes were drawn from bivariate or multivariate equations and separated out calculations of mean effect sizes accordingly. Second, we also coded for variations in methodological, sanction, and sociodemographic characteristics in the primary studies. These study characteristics are discussed next.

C. Moderators of Effect Size

The primary goal of a meta-analysis is to determine an overall mean effect size. In our analysis, this overall effect tells whether primary studies tend to find that incarceration reduces reoffending relative to noncustodial sanctions. Another question that meta-analyses can address is whether the size and direction of effect sizes vary significantly based on characteristics of individual studies and statistical models (Hall and Rosenthal 1991; Borenstein et al. 2009). These moderation analyses are particularly important when there is heterogeneity among primary studies. As discussed in Section II, the literature on the effectiveness of custodial sanctions is replete with discrepant findings. We also described potential reasons for such heterogeneity, including variations in the research designs used by the original researchers (e.g., regression, PSM, natural experiment; the specific covariates adjusted for), the sanctions served by participants (e.g., type and length of sanction), and other sociodemographic characteristics germane to the generality of custody's effects on reoffending (e.g., demographic distributions, country, time period). A large, heterogeneous sample of effect sizes permitted us to code for and analyze whether effect sizes were indeed moderated by those characteristics. As detailed in Section IV, the vast majority of these characteristics were not statistically significant moderators. In other words, regardless of the type of study design, sanction, or sample, incarceration has a null or criminogenic effect on reoffending. Below we detail how these characteristics were coded for each individual effect size in the sample.

1. *Research Design.* Several aspects of research design and model specification were coded for each effect size included. We included a categorical indicator for the overall *study design*, the statistical model used to calculate each effect size, or both. The types of designs coded for included those in which no control or matching variables were used (49.3 percent), multivariate regression models (14.8 percent), basic matching techniques (e.g., exact matching; 3.8 percent), propensity score matching or inverse probability of

treatment weighting (24.2 percent), natural experiments (e.g., using random judge assignment as an instrumental variable; 7.0 percent), and RCTs (0.9 percent). *Publication type* distinguishes studies released as peer-reviewed journal articles (50.0 percent), state or local reports (34.8 percent), federal reports (5.9 percent), theses or dissertations (1.7 percent), book chapters (1.0 percent), or any other type of document (e.g., unpublished working papers; 6.6 percent). The total *sample size* was also coded for each effect size. Categories included fewer than 100 offenders (2.5 percent), 100 to 499 (21.4 percent), 500 to 999 (11.2 percent), 1,000 to 4,999 (13.9 percent), 5,000 to 9,999 (15.3 percent), 10,000 to 49,999 (25.8 percent), 50,000 to 99,999 (5.8 percent), and greater than 100,000 offenders (4.2 percent).

Reoffending measure captures the type of reoffending by which custodial and noncustodial groups were compared. These include new convictions (42.7 percent), arrests or charges (30.1 percent), reincarceration (22.6 percent), technical violations (1.6 percent), mixed measures (0.3 percent), and other types of reoffending (e.g., self-reported offending; 2.7 percent). *Length of follow-up* reflects the amount of time samples were tracked for subsequent reoffending incidents: one year or less (25.3 percent), more than one to two years (21.5 percent), more than two to three years (35.8 percent), more than three to four years (4.3 percent), or greater than four years (13.1 percent). Finally, for each effect size, dichotomous indicators for a wide range of possible confounds were coded, with scores of 1 indicating the focal characteristic was either matched on prior to analysis or statistically controlled for in the analysis, and scores of 0 indicating the focal characteristics were neither matched on nor controlled for. The characteristics coded for included age, gender, marital status, employment status, education level, socioeconomic status, race or ethnicity, type of conviction offense, age at first offense, prior record, substance abuse, mental health problems, risk level, and length of sentence.

2. *Sanction Characteristics.* The types of noncustodial and custodial sentences served by samples were also coded. In terms of *noncustodial sanction type*, the majority of effect sizes were derived from samples sentenced to probation (54.0 percent), followed by intensive probation (15.2 percent), community service or fines (7.5 percent), suspended sentences or dismissals (6.0 percent), electronic monitoring or house arrest (3.1 percent), or an explicitly treatment-focused noncustodial sanction (e.g., community-based drug treatment; 1.7 percent). Approximately 13 percent of effect sizes were calculated from studies in which the type of noncustodial sanction was categorized as “Other,” capturing both unspecified noncustodial sanctions

and low-frequency sanctions (e.g., restorative justice). For the *custodial sanction type* variable, the majority of effect sizes came from studies in which the custodial group was sentenced to prison (63.7 percent), followed by juvenile detention (10.8 percent), jail (8.7 percent), boot camp or shock incarceration (3.8 percent), or a secure residential facility (0.9 percent). A further 12.1 percent were coded as “Other,” which indicates unspecified or mixed sanction types (e.g., where the custodial sample was sentenced to either prison or jail). The *time in custody* moderator variable reflects the mean length of time that offenders in each sample spent in a custodial setting. The majority of effect sizes were drawn from studies that did not report the mean length of time in custody (73.8 percent). The remaining studies reported an average length of incarceration of less than one month (1.4 percent), between one month and less than six months (7.2 percent), between six months and less than one year in custody (12.8 percent), between one year and less than two years (2.0 percent), or two or more years (2.7 percent).

3. *Sociodemographic Characteristics.* Several other characteristics relevant to the generality of custody’s effects on reoffending were coded. For *age composition*, most samples were exclusively comprised of adults (66.3 percent), followed by samples that were exclusively juvenile (16.2 percent), more than 80 percent adult (2.7 percent), or more than 80 percent juvenile (0.5 percent). A further 14.3 percent of effect sizes came from studies that did not report the age composition of their samples. For *gender composition*, the majority of samples were either exclusively male (15.9 percent) or comprised of more than 80 percent males (33.5 percent), followed by those that were a “mixed group” (i.e., less than 80 percent male; 13.4 percent), or exclusively female (3.1 percent). A further 34.2 percent of effect sizes were derived from studies in which gender composition was unreported. *Publication decade* taps the decade during which the study was published (1960s = 2.0 percent; 1970s = 0.5 percent; 1980s = 3.8 percent; 1990s = 14.2 percent; 2000s = 13.2 percent; 2010s = 66.3 percent). *Study location* refers to the country from which the sample was drawn, including the United States (78.3 percent), Canada (1.3 percent), the United Kingdom (5.3 percent), Australia (3.4 percent), Nordic countries (i.e., Denmark, Finland, and Norway; 4.6 percent), the Netherlands (3.8 percent), or some other country (3.3 percent).

D. *Analytic Plan*

Once effect sizes and moderating characteristics from each study were coded, meta-analytic procedures were used to synthesize information across

studies. Specifically, we employed the MLM procedures described by Hox, Moerbeek, and van der Schoot (2018) and used in other recent meta-analyses (e.g., Pratt et al. 2014; Pyrooz et al. 2016; Myers et al. 2020) to estimate the mean effects of custodial versus noncustodial sanctions on reoffending. A two-level MLM framework was appropriate given that effect size estimates are nested; that is, Level 1 of the data corresponds to individual statistical models producing each effect size ($N = 981$), while Level 2 corresponds to the studies from which (often multiple) effect sizes were drawn ($k = 116$). At Level 1, then, effect sizes within studies often share the same samples and methods of data collection, while assessing different outcomes and with different model specifications. Without accounting for this hierarchical nature of the data, the assumptions of the statistical independence of observations and uncorrelated error are violated (Snijders and Bosker 2012), thus increasing the likelihood of biased tests of statistical significance because of artificially deflated standard errors and narrow confidence intervals (Kreft and de Leeuw 1998). However, the MLM framework resolved these issues and accounted for both within- and between-study sources of dependence through the inclusion of a unique random effect for each organizational unit (Pratt et al. 2014; Turanovic et al. 2021). Calculation of the intraclass correlation coefficient for the full analytic sample provides further evidence of the necessity to account for both sources of variation; 47.8 percent of the variance in effect size estimates was within studies ($\sigma^2 = .0065$, $p < .001$), while 52.2 percent was between studies ($\sigma^2 = .0071$, $p < .001$).

Another issue with hierarchical meta-analytic data is that a portion of the variance of Level 1 effect size estimates is assumed to be known (Hox and de Leeuw 2003). Explicitly accounting for this variance is crucial given that effect size estimates are drawn from other studies that vary in precision (e.g., because of differences in sample size or model specification). To do so, we calculated standard errors for each effect size using $\sigma = \sqrt{1/(n-3)}$ for bivariate effect size estimates and $\sigma = \text{Fisher's } z(r)/(b/SE)$ for multivariate effect size estimates (Lipsey and Wilson 2001). These effect size standard errors were incorporated into the random part of the Level 1 equation (StataCorp 2013; Hox, Moerbeek, and van der Schoot 2018; see also Pratt et al. 2014; Pyrooz et al. 2016; Myers et al. 2020), thus enabling effect size estimates to be weighted by their precision and models to account for within-study variation beyond what is implied by their known variance.

With these considerations in mind, the analyses presented below proceeded in four stages. First, we estimated the overall mean effect of custodial sanctions (compared with noncustodial sanctions) on reoffending. We

examined these mean effect sizes across the entire sample of 981 effect size estimates, as well as when looking only at estimates derived from bivariate or multivariate models. Second, we conducted a series of bivariate moderator analyses to determine whether the overall mean effect of custodial sanctions on reoffending was robust across variations in characteristics of individual studies and statistical models. All bivariate moderator analyses were conducted by entering only the focal moderator variable into the regression equation. Third, we examined the influence of significant moderators from the previous step when considered together in a multivariate meta-regression model, both with the full sample and restricted to effect sizes from multivariate models. Fourth, we examined whether effect sizes differed between studies that met a set of methodological “best-case” criteria and those studies that did not meet those strict criteria. All analyses were conducted in Stata 15 using the *-meglm-* command with maximum likelihood estimation.

IV. Assessing the Effects of Custodial Sanctions: A Meta-Analysis

This section presents the results of our meta-analysis of research comparing the outcomes of offenders sentenced to custodial versus noncustodial sanctions. Part A describes findings regarding the overall mean effect of custodial sanctions in the entire sample of effect size estimates. We also present average effect sizes broken down by the type of research design used to generate each effect size. These analyses reveal that, on average, sentencing offenders to custodial sanctions has either null or criminogenic effects on reoffending. Part B discusses findings on whether the effects of custodial sanctions vary significantly by the statistical models or research designs used in primary studies, the sanctions examined, and other demographic characteristics. As we show, although there is some evidence of moderation, the null or criminogenic effects of imprisonment persist regardless of all these characteristics.

A. Overall Strength of Effects

The first objective of the meta-analytic review was to determine the overall mean effects of custodial sanctions on reoffending. These effects are reported in table 1. Estimates were obtained from unconditional models—that is, models in which no other predictors were entered into

TABLE 1
Mean Estimates for the Effect of Custodial Sanctions on Recidivism

Model Estimation	Mean ESE	95 Percent CI	Min	Max	ICC
Full sample (981)	.079***	.061 to .096	-.319	.572	.522
No controls/matching (484)	.098***	.069 to .127	-.319	.572	.478
Multivariate (497)	.067***	.048 to .086	-.271	.484	.745

NOTE.—Number of effect sizes are in parentheses. CI = confidence interval; ESE = effect size estimate; ICC = intraclass correlation coefficient.

- * $p < .05$.
- ** $p < .01$.
- *** $p < .001$ (two-tailed test).

the equations. However, given the variance-known, multilevel framework used in the analyses, effect sizes that are more precise received a larger weight (see Pratt et al. 2014; Hox, Moerbeek, and van der Schoot 2018). Values in the “Mean ES” column in the table can be interpreted as the average correlation between a custodial sentence and reoffending. Considering the full sample of 981 effect size estimates drawn from 116 primary studies, table 1 shows that the mean correlation between a custodial sentence and reoffending was .079. This effect size means that, on average, being sentenced to custody increases reoffending.

The size of this criminogenic effect is small when compared against predictors of crime that have been meta-analyzed previously. For example, Bonta and Andrews (2017) found that the average correlation between adhering to the principles of effective correctional treatment and reoffending is $-.260$. Other studies have likewise shown that self-control (mean correlation = $.257$; Pratt and Cullen 2000), gang membership (mean correlation = $.227$; Pyrooz et al. 2016), and peer influence (mean correlation = $.321$; Gallupe, McLevey, and Brown 2019) have much stronger effects on crime than sentencing individuals to terms of imprisonment. Although the effect of custodial sanctions on reoffending is smaller than these other predictors of crime, it may nonetheless be substantively important. A mean correlation of approximately $.080$ translates into an 8 percentage point difference in reoffending between those sentenced to custodial and noncustodial sanctions (Bonta and Andrews 2017; see also Randolph and Edmondson 2005). Thus, assuming that 46 percent of the comparison (noncustodial) group reoffended, the percent reoffending in the custodial sanction group would be 54 percent. When extrapolated to a group the size of the incarcerated

population in the United States, this difference is meaningful. At the very least, from a policy perspective, this finding indicates that custodial sanctions do not reduce recidivism.

Table 1 also reports mean effect sizes when broken down by whether primary studies relied on bivariate or multivariate analyses. Unsurprisingly, the largest criminogenic effects were observed in bivariate models that did not make use of any statistical controls or match participants on a set of covariates. In other words, no attempts were made in these analyses to account for the preexisting differences between individuals sentenced to custody and those sentenced to a noncustodial sanction such as probation. The mean correlation between a sentence of custody and reoffending in these bivariate models was .098. In multivariate models (e.g., multiple regression, matching, propensity score analysis), however, the mean correlation was approximately .067. Although this effect size is approximately 32 percent smaller than that observed in bivariate models, multivariate models still point to a small but statistically significant criminogenic effect of imprisonment on reoffending.

B. Robustness of Effects

The key takeaway from the results presented thus far is that, on average, custodial sanctions appear to have a small criminogenic effect on reoffending. However, given that this finding represents an *average* effect across many different studies and statistical models, it is important to determine whether there are conditions under which the overall finding changes. It is possible, for example, that imprisonment acts as a deterrent for juvenile offenders but is criminogenic for adult offenders, or that prisons in the United States are far more criminogenic than prisons in European countries. Likewise, it has been suggested that studies with poor methodological quality may produce vastly different findings than studies of strong methodological quality (Nagin, Cullen, and Jonson 2009; Villettaz, Gillieron, and Killias 2015). In light of these possibilities, we followed the common practice of conducting meta-analytic moderation analyses (see, e.g., Pratt et al. 2014; Pyrooz et al. 2016; Gallupe et al. 2019). These analyses tested whether the overall mean effect of custody was indeed sensitive to study-to-study variations in methodological quality, the sanctions examined, and the sociodemographic characteristics of samples.

The moderator analyses proceeded in three phases, each of which is discussed further below. First, we separately examined the influence of each moderator variable on effect size estimates. The goal of this phase was to

determine which characteristics of studies predict variation in effect size estimates when examined at the bivariate level. Second, statistically significant moderators of effect sizes were included together in two different meta-regression models. The purpose of this phase was to test whether moderators maintained a significant influence on effect size estimates after controlling for the influences of the other moderators. In the first model, these meta-regression tests were conducted on the full sample of effect sizes ($N = 981$), while the second was restricted to effect sizes that came from multivariate models that accounted for the differences between offenders sentenced to custody and those to noncustodial settings ($N = 497$). Third, we examined whether effect sizes from an even smaller pool of high-quality studies ($N = 226$) differed significantly from those that did not meet rigorous standards ($N = 755$). The results presented below indicate that, through each of the successive analytic phases, fewer characteristics of studies emerge as statistically significant moderators of effect size estimates. Although there are some characteristics that maintain a significant moderating effect through all phases, the ultimate finding is that imprisonment has a null or slight criminogenic effect on reoffending regardless of variations in methodological quality, the sanctions evaluated, and sociodemographic characteristics of the samples. In other words, we find no conditions under which custody tends to reduce reoffending.

1. *Bivariate Moderator Analyses.* In the first set of moderation analyses, the effect of each study characteristic on effect sizes was assessed separately. These analyses were conducted with the entire sample of 981 effect size estimates. Findings from these models are presented in tables 2–4. The intercepts in these tables are interpreted as the mean effect size estimate when the moderator is set at its reference category—the category listed first for each variable. Estimates displayed in the coefficient columns should likewise be interpreted as the change in effect size when the moderator variable is moved from the reference category to another category of interest. For example, inspection of the publication type variable in table 2 shows that the mean effect size estimate for studies published in peer-reviewed journals (the reference category) was .066. The positive coefficient for studies released in a state or local reports indicates a larger criminogenic effect (i.e., $.066 + .040 = .106$), although the difference between effect sizes in peer-reviewed journals and state or local reports was not statistically significant.

The results in tables 2–4 suggest that some methodological, sanction, and sociodemographic characteristics do moderate effect sizes. However, despite variation away from the overall mean effects reported in table 1,

TABLE 2
Bivariate Moderator Analyses for the Impacts of Methodological
Characteristics on Effect Size Estimates

Moderator Variable	Coefficient	SE	z-Value	Intercept
Study design:				.102***
No controls/matching (484)				
Multivariate regression (145)	-.054	.015	-3.59***	
Basic matching (37)	-.020	.029	-.69	
PSM/IPTW (237)	-.007	.014	-.54	
Natural experiment (69)	-.107	.024	-4.51***	
RCT (9)	-.047	.054	-.87	
Publication type:				.066***
Peer-reviewed article (501)				
State/local report (334)	.040	.024	1.67	
Federal report (57)	.047	.038	1.26	
Thesis/dissertation (16)	-.033	.050	-.66	
Book chapter (10)	.159	.064	2.49*	
Other (63)	.020	.040	.52	
Sample size:				.086***
100 to 499 (210)				
<100 (24)	.003	.038	.08	
500 to 999 (110)	-.000	.014	-.01	
1,000 to 4,999 (136)	-.012	.017	-.69	
5,000 to 9,999 (150)	-.003	.022	-.16	
10,000 to 49,999 (253)	-.008	.017	-.46	
50,000 to 99,999 (57)	-.064	.024	-2.73**	
>100,000 (41)	-.066	.023	-2.86**	
Recidivism measure:				.091***
Arrest/charge (291)				
Conviction (426)	-.023	.009	-2.55*	
Reincarceration (219)	-.024	.009	-2.53*	
Technical violation (15)	.048	.032	1.53	
Other (27)	-.009	.025	-.34	
Length of follow-up:				.070***
≤1 year (245)				
>1 year to ≤2 years (206)	.011	.010	1.13	
> 2 years to ≤ 3 years (347)	.011	.010	1.03	
>3 years to ≤4 years (44)	.029	.028	1.03	
>4 years (133)	.017	.017	1.01	
Statistical controls/matching variables:				
Age (419)	-.041	.011	-3.74***	.098***
Gender (394)	-.037	.012	-3.08**	.094***
Marital status (67)	.018	.022	.79	.077***
Employment status (79)	.004	.019	.21	.078***
Education (140)	.032	.016	1.94	.074***
Socioeconomic status (65)	.035	.021	1.70	.077***
Race/ethnicity (349)	-.012	.012	-1.01	.083***
Conviction offense (313)	-.028	.013	-2.17*	.089***

TABLE 2 (Continued)

Moderator Variable	Coefficient	SE	z-Value	Intercept
Age at first offense (54)	.017	.022	.78	.077***
Prior record (392)	.001	.012	.11	.078***
Substance abuse (77)	.044	.021	2.12*	.075***
Mental health (36)	−.026	.062	−.43	.079***
Risk level (65)	−.045	.022	−2.02*	.084***
Sentence length (98)	−.066	.018	−3.70***	.083***

NOTE.—Estimates are based on the full sample ($N = 981$). The frequencies of effect size estimates are in parentheses. Models were estimated separately for each moderator. IPTW = inverse probability of treatment weighting; PSM = propensity score matching; RCT = randomized control trial; SE = standard error.

* $p < .05$.
** $p < .01$.
*** $p < .001$ (two-tailed test).

the effects of custody remain null or criminogenic. In table 2, findings related to the possible moderating effects of variations in methodological characteristics are presented. These results illustrate that effect sizes drawn from studies using multivariate regression models and natural experiments were significantly less than those drawn from studies that used no controls or matching variables. For example, the mean correlation between custody and reoffending in the uncontrolled studies was .102, indicating a small but statistically significant criminogenic effect of imprisonment. This effect was reduced to .048 and −.005 in multivariate regression and natural experiment studies, respectively. Effect sizes from studies that used basic matching or propensity score techniques did not differ significantly from those of uncontrolled studies.

Table 2 also indicates that variations in sample size had little influence on effect sizes until samples reached 50,000 offenders. Specifically, studies with samples of between 50,000 and 99,999 and greater than 100,000 had significantly smaller effect sizes than those with 100 to 499 participants. The type of publication in which results were presented made little difference in effect size estimates. The only category that differed significantly from peer-reviewed journal articles was results appearing in book chapters. However, this category contained only 10 effect size estimates drawn from two studies, so this result should be interpreted with caution. In terms of the type of reoffending assessed, effect size estimates were smaller for both convictions and reincarcerations when compared to models examining

arrests or charges. The length of postsanction follow-up, which varied quite widely across the entire sample, did not emerge as a moderating factor. Finally, we also examined whether controlling for or matching participants on a host of theoretically relevant confounds impacted effect size estimates. As table 2 indicates, models that included controls for or matched on age, gender, conviction offense type, risk level, and sentence length reported smaller effects of custody on reoffending than models that did not. By contrast, models that controlled for or matched on participants' history of substance abuse produced larger effect size estimates.

Table 3 displays the findings of moderator analyses examining the impact of variations in sanction characteristics on effect size estimates. As noted in Section III, the sample included effect sizes from studies that examined a range of different forms of custodial and noncustodial sanctions. The most common form of custodial sanction examined in primary studies was prison, accounting for 63.9 percent of the sample of effect size estimates ($N = 627$). Among studies that examined the effects of prison, the intercept column in table 3 shows that the mean correlation between custody and reoffending was .053. This finding indicates that serving a term in prison has a small, but statistically significant, criminogenic effect on post-release reoffending. Table 3 also shows that relative to studies examining prison, being sentenced to jail or juvenile detention was associated with a larger criminogenic effect of imprisonment. In terms of the various types of noncustodial sanctions, probation was the most common form represented in the sample of effect sizes (53.5 percent; $N = 525$). For studies that used probationers as a comparison group, the mean correlation between custody and reoffending was .113, which again demonstrates that serving time in custodial settings has a criminogenic effect. The results reported in table 3 reveal that, relative to studies examining probation, the effects of custody were smaller in magnitude—albeit still criminogenic—when comparison offenders were sentenced to receive intensive probation, community service or fines, a suspension or dismissal, or some other noncustodial disposition. Table 3 also shows that variations in the amount of time spent in custody generally had no moderating influence on effect size estimates. The sole exception was that, when compared with studies that did not report the length of custody, the mean criminogenic effect of custodial sanctions was smaller in studies examining individuals sentenced to between one and less than six months in prison.

Table 4 presents the results of moderation analyses examining socio-demographic characteristics that might affect the generality of custody's

TABLE 3
Bivariate Moderator Analyses for the Impact of Sanction Characteristics
on Effect Size Estimates

Moderator Variable	Coefficient	SE	z-Value	Intercept
Custodial sanction type:				.053***
Prison (627)				
Jail (85)	.072	.015	4.75***	
Juvenile detention (104)	.096	.018	5.41***	
Boot camp/shock (36)	-.032	.037	-.84	
Residential treatment (11)	.030	.062	.49	
Other (118)	.046	.026	1.74	
Noncustodial sanction type:				.113***
Probation (525)				
Intensive probation (148)	-.096	.012	-8.04***	
EM/house arrest (34)	-.038	.029	-1.30	
Community service/fine (79)	-.048	.023	-2.09*	
Tx-focused NCS (16)	-.017	.040	-.43	
Suspended/dismissal (57)	-.053	.019	-2.74**	
Other (122)	-.061	.020	-3.05**	
Time in custody:				.087***
Not reported (724)				
<1 month (14)	-.027	.027	-1.00	
1 to <6 months (71)	-.093	.045	-2.09*	
6 months to <1 year (126)	.001	.024	.02	
1 to <2 years (20)	-.040	.038	-1.04	
≥2 years (26)	-.053	.069	-.77	

NOTE.—Estimates are based on the full sample ($N = 981$). The frequencies of effect size estimates are in parentheses. Models were estimated separately for each moderator. EM = electronic monitoring; NCS = noncustodial sanction; SE = standard error; Tx = treatment.

* $p < .05$.

** $p < .01$.

*** $p < .001$ (two-tailed test).

effects on reoffending. Several findings are noteworthy. First, results for age composition indicate that the average correlation between custody and reoffending was .065 in adult-only samples but increased to .112 in juvenile-only samples. Thus, imprisonment appeared to have larger criminogenic effects for juveniles than adults when no other moderators were included in the regression model. Second, studies using mixed-gender samples (80 percent or less male) tended to report larger criminogenic effects of custody than studies examining male-only samples. There was, however, no significant difference in mean effect sizes between exclusively male and exclusively female samples, although the number of effect sizes from female-only

TABLE 4
Bivariate Moderator Analyses for the Impact of Sociodemographic
Characteristics on Effect Size Estimates

Moderator Variable	Coefficient	SE	z-Value	Intercept
Age composition:				.065***
Exclusively adults (644)				
Exclusively juveniles (158)	.047	.014	3.37**	
>80% adults (230)	.015	.038	.39	
>80% juveniles (5)	.030	.077	.38	
Missing (144)	.040	.038	1.05	
Gender composition:				.067***
Exclusively males (159)				
Exclusively females (30)	.024	.026	.95	
>80% males (331)	.013	.020	.65	
Mixed (133)	.078	.012	6.30***	
Missing (328)	−.015	.021	−.75	
Publication decade:				.080***
2010s (655)				
2000s (130)	−.011	.024	−.48	
1990s (136)	−.022	.023	−.97	
1980s (36)	.034	.038	.90	
1970s (5)	.168	.071	2.37*	
1960s (19)	.018	.065	.28	
Study location:				.070***
United States (762)				
Canada (12)	.019	.054	.36	
United Kingdom (51)	.030	.040	.75	
Australia (33)	.049	.040	1.22	
Nordic countries (47)	−.022	.049	−.56	
The Netherlands (40)	.040	.042	.96	
Other (36)	.041	.342	1.21	

NOTE.—Estimates are based on the full sample ($N = 981$). The frequencies of effect size estimates are in parentheses. Models were estimated separately for each moderator. SE = standard error.

- * $p < .05$.
- ** $p < .01$.
- *** $p < .001$ (two-tailed test).

samples was quite small ($N = 30$). Third, neither the country from which data were drawn nor the decade during which studies were published generally had any significant association with the size of custody’s effects on reoffending.

To summarize the results thus far, the bivariate moderator analyses suggest that some aspects of the primary studies’ research designs, the sanctions

evaluated, and the sociodemographic characteristics of samples do account for variation in effect size estimates. Effect sizes tended to be smaller, albeit still null or criminogenic, in studies that used stronger research designs, accounted for more confounding factors related to reoffending (e.g., age, gender, risk level), and had larger sample sizes. Incarceration also appeared to be more criminogenic for juvenile offenders than for adults, and there were many differences in effect size when looking at variations in the specific forms of custodial and noncustodial sanctions (e.g., probation versus intensive probation, jail versus prison). However, factors such as the length of follow-up, amount of time spent in custody, the gender composition of samples, the decade of publication, and the country from which data were collected had no significant moderating influence on effect sizes. In other words, the effects of custody were general across those characteristics.

2. *Meta-Regression Moderator Analysis.* Although some characteristics emerged as significant moderators in the bivariate analyses, it is important to note that moderators may be correlated with each other (Lipsey 2003). Without accounting for the correlations between moderators, the relationships between them and effect sizes may be spurious and the magnitudes of their associations inflated or deflated. For example, we explained above that stronger research designs tend to produce smaller effect sizes than weaker designs. However, it is possible that there is nothing inherent in the design itself that affects effect sizes, but rather that stronger designs tend to account for a broader range of confounders (e.g., age, gender, prior record). In order to evaluate whether those sorts of moderator confounding issues existed in our bivariate analyses, the next step was to examine the statistically significant moderators from the initial bivariate analyses together in meta-regression models. This regression approach provides a stronger test of moderation given its ability to isolate the effect of a given moderator after controlling for the influence of other moderating variables. As detailed below, the meta-regression approach was used to assess moderation in both the full sample of effect size estimates ($N = 981$) and a subsample of effect sizes from multivariate studies that accounted for at least some confounding variables through random assignment, matching, or statistical controls ($N = 497$).

To conduct the meta-regression analyses, it was necessary to initially check that multicollinearity among the variables was not a concern. Multicollinearity exists when there are very strong correlations between the independent variables included in a regression model. Strong correlations can indicate that the variables are measuring the same construct, making

it difficult to isolate the effect of any single variable (Weisburd and Britt 2014). To check for this problem, we examined the data for correlations of greater than .750 between moderating variables and variance inflation factors above 4.00 (Tabachnick and Fidell 2007; Weisburd and Britt 2014). In the full sample of effect size estimates ($N = 981$), diagnostic tests indicated multicollinearity between the statistical control or matching indicators. For example, there were very strong correlations between the indicators of whether studies had included age and race ($r = .740$), prior record ($r = .756$), and current offense-type ($r = .744$) in their statistical models. Given the presence of multicollinearity between the statistical control or matching indicators, those variables were excluded from the meta-regression with the full sample.

Findings from the meta-regression analysis with the full sample are presented in table 5 (Model 1). The results were quite similar to the bivariate moderator analyses reported above in tables 2–4. Specifically, after accounting for the influence of other moderator variables, effect sizes from multivariate regression models and natural experiments tended to be smaller than effect sizes from studies that did not use statistical controls / matching variables. There were no significant differences between uncontrolled or unmatched studies and those using basic matching or propensity score techniques. Likewise, there were significant differences in effect sizes depending on the specific types of custodial and noncustodial sanctions examined. For example, studies examining jail, juvenile detention, and residential treatment tended to report larger criminogenic effects of custody than studies examining prison. Studies examining intensive probation, community service or fines, and suspended sentences or dismissals also reported smaller criminogenic effects of custody than studies examining regular probation. Another similarity to the bivariate moderator analyses was that the full-sample meta-regression indicated studies examining convictions and reincarceration tended to report smaller effect sizes than studies examining rearrests. There were, however, two important differences in the full-sample meta-regression model. First, in the bivariate analyses, the criminogenic effects of custodial sanctions were larger in studies with juvenile-only samples than in adult-only samples. In the meta-regression context, this effect was no longer significant. This finding suggests that once other aspects of studies are accounted for (e.g., research design, covariates adjusted for, sample size), the outcome of incarceration is generally the same for both adults and juveniles. Second, large sample sizes (i.e., greater than 50,000) were no longer a significant predictor of effect size estimates in the meta-regression context.

The full sample meta-regression (table 5, Model 1) analysis revealed that fewer characteristics of studies moderate effect size estimates once other variables are controlled for. To probe this issue further and provide an even stronger test of moderation, we thus estimated a second meta-regression model with a subsample of effect sizes from multivariate models that accounted for confounding variables through random assignment, matching, or statistical controls ($N = 497$). In this reduced sample, correlations between the statistical control or matching variable indicators were no longer problematic (e.g., correlation between age and prior record was $-.040$), and none of the variance inflator factors exceeded 4.00; thus, these indicators were included in analyses of the multivariate effect size estimates. As the results in table 5 (Model 2) illustrate, there are several important findings when examining moderators of effect size within the multivariate-only sample. First, there are still differences in effect size based on the overarching research design used in the primary studies. Effect sizes from models using basic matching and propensity score techniques were significantly larger than those from multivariate regression models, while those from natural experiments were smaller. Similar to the earlier moderator analyses, the reductions in effect size for natural experiments were sizable. However, even with these reductions, the mean effect of custody is null or slightly criminogenic.

Second, the types of statistical controls or matching variables used in models were less predictive of effect size estimates after accounting for other moderators in the multivariate sample. Specifically, only age, socioeconomic status, and risk level remained statistically significant in the meta-regression model. Studies that included any of those three variables tended to report smaller effect sizes than studies that did not include them. However, whether studies included other factors such as sentence length, conviction offense, and gender was no longer predictive of effect size in the multivariate-only sample. Third, when looking at the type of reoffending measure used by researchers, effect sizes from studies examining convictions remained smaller than those using arrest after accounting for other moderators. Finally, the specific types of sanctions examined—both custodial and noncustodial—made less of an impact on effect size estimates in multivariate models. For example, in contrast to the analyses of the full sample that included bivariate effect size estimates (table 5, Model 1), multivariate models that examined jail and juvenile detention did not have significantly different effect sizes from those examining prison as a custodial sanction. In other words, better-controlled models tend to find that custody

TABLE 5
Multivariate Meta-Regression of Effect Size Estimates on
Methodological, Sanction, and Sociodemographic Variations

Moderator Variables	Model 1 Full Sample		Model 2 Multivariate Sample	
	Coefficient	SE	Coefficient	SE
Fixed Effects				
Model Intercept	.125***	.024	.086**	.029
Study design:				
No controls/matching				
Multivariate regression	−.067***	.015	(Ref)	
Basic matching	−.035	.030	.066*	.027
PSM/IPTW	.001	.015	.036*	.018
Natural experiment	−.129***	.023	−.069***	.011
RCT	−.050	.057	.002	.052
Sample size:				
100 to 499				
<100	.033	.037	.009	.042
500 to 999	.015	.014	.043**	.013
1,000 to 4,999	.001	.017	−.006	.015
5,000 to 9,999	.009	.022	.014	.017
10,000 to 49,999	.005	.018	−.011	.016
50,000 to 99,999	−.027	.030	.017	.031
>100,000	−.017	.026	.012	.022
Recidivism measure:				
Arrest/charge				
Conviction	−.026**	.008	−.034**	.013
Reincarceration	−.026**	.009	−.005	.014
Technical violation	.060*	.029	.019	.036
Other	.023	.086	.007	.016
Statistical controls/matching variables:				
Age			−.053*	.026
Gender			.020	.026
Marital status			−.010	.026
Employment status			.026	.032
Education			−.010	.026
Socioeconomic status			.065*	.028
Race/ethnicity			−.015	.022
Conviction offense			.029	.021
Age at first offense			.028	.018
Prior record			.031	.018
Substance abuse			.000	.028
Mental health			.015	.056
Risk level			−.076**	.026
Sentence length			−.019	.015
Custodial sanction type:				
Prison				

TABLE 5 (Continued)

Moderator Variables	Model 1 Full Sample		Model 2 Multivariate Sample	
	Coefficient	SE	Coefficient	SE
Jail	.084***	.014	−.007	.015
Juvenile detention	.072*	.032	−.029	.037
Boot camp/shock	−.042	.037	−.088**	.033
Residential treatment	.101***	.027	.025	.055
Other	.017	.062	.008	.026
Noncustodial sanction type:				
Probation				
Intensive probation	−.101***	.011	−.001	.012
EM/house arrest	−.029	.028	−.006	.023
Community service/fine	−.046*	.023	−.027	.024
Tx-focused NCS	−.030	.041	.021	.033
Suspended/dismissal	−.056**	.056	−.063***	.012
Other	−.073***	.021	−.012	.021
Age composition:				
Exclusively adults				
Exclusively juveniles	.012	.023	−.008	.017
>80% adults	.023	.038	.005	.050
>80% juveniles	−.051	.078	.010	.067
Missing	−.007	.042
Gender composition:				
Exclusively males				
Exclusively females	.011	.021	.001	.013
>80% males	.012	.020	.013	.015
Mixed	−.068*	.031	−.029	.022
Missing	−.036	.026	−.037	.027
Number of effect sizes (Level 2)	−.000	.001	−.000	.001
Random effects:				
Level 1 (Effect size estimates):				
Variance between models	.00499	.00028	.00131	.00014
Variance explained	24.11%		36.30%	
Level 2 (Study):				
Variance between studies	.00680	.00131	.00402	.00094
Variance explained	4.36%		35.14%	

NOTE.—Full sample $N = 981$; Multivariate sample $N = 497$. *Abbreviation:* SE = standard error.

* $p < .05$.
** $p < .01$.
*** $p < .001$ (two-tailed test).

is ineffective or criminogenic regardless of which specific form of custody is examined. Coefficients for the noncustodial sanction type indicators similarly show that custody has null or criminogenic effect regardless of the type of noncustodial sanction served by comparison groups.

Taken together, our meta-regression analyses reveal that very few factors moderate effect sizes capturing the influence of custodial sanctions on reoffending. In other words, the analyses show that the null or slight criminogenic effect of custody is general rather than specific to particular research designs, varieties of custodial or noncustodial sanctions, or socio-demographic characteristics of samples. The minimal effects of custody on reoffending are the same for both males and females and both adults and juveniles. The effects of custody also do not vary much based on whether researchers study prison, jail, or other forms of custodial sanctions, or whether they compare custody to probation, intensive probation, electronic monitoring, or other noncustodial sanctions. The overarching study design (e.g., uncontrolled, propensity score analysis, natural experiment) and type of reoffending measured did have significant moderating influences on effect size estimates in both meta-regression models. However, none of these moderation effects were large enough to change the overall finding of a null or criminogenic effect of custody on reoffending.

3. *Best-Case Studies.* As a final step in the analyses, we sought to identify the mean effect size estimate in studies that met the methodological “best-case” criteria. As discussed previously, Nagin, Cullen, and Jonson (2009; see also Villettaz, Gillieron, and Killias 2015) note that although RCTs are the gold standard for evaluating treatment effectiveness, they are often neither viable nor ethical when sanctioning offenders. At a minimum, then, they urge researchers to implement quasi-experimental designs that can account for the influences of offenders’ age, race or ethnicity, gender, current offense, and prior record on both sanction assignment and reoffending. We therefore conducted a supplementary analysis that compared mean effect size estimates drawn from studies that implemented a natural experiment or RCT, or used propensity score techniques to account for at least the five factors noted above, with studies that did meet these criteria. As expected, the mean correlation between custody and reoffending among the 226 effect sizes that met these criteria ($r = .050$) was approximately 40% smaller than the mean among the 755 effect sizes that did not ($r = .083$). However, these results illustrate that even in studies that meet fairly rigorous methodological criteria, a weak but statistically significant criminogenic effect of custodial sanctions is observed.

We thus reach the same conclusion as all of the other analyses presented in this essay: imprisonment has either no effect or makes reoffending outcomes worse when compared with noncustodial sanctions such as probation, electronic monitoring, or otherwise. This finding echoes those of prior reviews of the literature by Smith, Goggin, and Gendreau (2002), Nagin, Cullen, and Jonson (2009), Jonson (2010), and Villettaz, Gillieron, and Killias (2015). However, we have extended those reviews to show that the null or criminogenic effect of custody exists regardless of the specific varieties of custodial and noncustodial sanctions that are compared; the age and gender distributions of participants; where the data were collected; the type of reoffending measured and for how long; and variations in the research designs of primary studies. Although effect sizes did vary significantly by research design and the type of reoffending measured across all analyses, the substantive conclusion of a null or criminogenic effect did not change. There does not appear to be a particular group of offenders that is more deterrable by incarceration than others, nor a particular type of research design that points to a deterrent effect when others do not (for a similar conclusion, see Loeffler and Nagin 2021).

V. Conclusion

In 1976, Gordon Hawkins incorporated into his short but illuminating book, *The Prison: Policy and Practice*, a chapter on “The Effects of Imprisonment.” He reviewed commentary on why prisons might or might not be “schools of crime.” For those on either side of the debate, some consolation could be drawn from knowing that the “belief that all who enter prison are ineluctably doomed to deterioration” has no more basis than the “antithetical idea” that prisons might “transform all offenders into model citizens” (p. 80). In moving forward, he argued that “critical evaluation of penal measures is an essential precondition to rational and effective policy formulation and planning” (1976, pp. 175–76). Alas, a “lack of knowledge” existed, which prompted Hawkins (1976, pp. 175–76) to caution, “There is depressingly little methodologically rigorous evaluative research available to guide our efforts.”

Five years later, Hawkins joined with Michael Sherman in another slim volume taking stock of “imprisonment in America.” Sherman and Hawkins (1981, p. 1) documented troubling trends, noting that the nation’s prisons and jails may house “more than a half million adults,” a “fraction of the citizenry larger than that of any other Western nation.” They noted that

conversations in the US Senate ranged from Strom Thurmond declaring that “overcrowded conditions in our prisons have become a national crisis” to Joe Biden introducing federal legislation to fund the construction of more prison cells at the state and local level (1981, p. 3). They decried this “crisis mentality,” however, arguing that the opportunity still existed to “choose the future” of American corrections. They argued for a balanced approach that incorporated conservative and liberal values and that would stabilize prison populations. Sherman and Hawkins (1981, p. 132) trumpeted this approach as a “principled stance” and concluded that “it seems literally a shame not to try.”

Four decades later, the consequences of “not trying” are palpable. The era of mass imprisonment that unfolded was costly, negatively affected the lives of individuals and communities, had racially disparate effects, and left the nation with an incarcerated population of approximately two million—a figure that makes the half million inmates of 1981 seem almost quaint (Clear 2007; Alexander 2010; Simon 2014; Aviram 2015; Pratt 2019). Still, Hawkins’s writings are useful not only as a reminder of the correctional past that existed when his books were written but also for the lessons they teach that remain pertinent today. Two seem most important.

First, moments exist when the opportunity to choose a new correctional future is most propitious. Historians can settle whether 1981 was one of these occasions. It is clearer, however, that we are now at a possible correctional turning point where the past does not have to be a prelude to the future (Petersilia and Cullen 2015). The seemingly ineluctable growth in prisons halted around 2009 and has since trended gradually downward (Maruschak and Minton 2020). Public punitiveness is in prolonged decline (Enns 2016; Pickett 2019), and public support for correctional reform, including alternatives to incarceration, is widespread (Sundt et al. 2015; Thielo et al. 2016; Butler et al. 2020).

Second, Hawkins is correct that “rational” policy and practice should be informed by research. The lack of knowledge was near complete when *The Prison* was written, but this is not the case today, when “evidence-based corrections” has evolved. Substantial scientific evidence, much of it based on evaluation research, provides direction on what does and does not work to change the behavior of justice-involved individuals (Lipsey and Cullen 2007; Bonta and Andrews 2017; more generally, see Cullen and Jonson 2017). This literature is clear in showing the limits of punishment-oriented interventions. Among this category of punitive sanctions, the data reveal that custodial placements, including in prison settings, are not effective

in reducing future reoffending. We explore below the implications of this conclusion for criminology and public policy.

A. A Criminological Fact

Over the past two decades, research has steadily accumulated assessing the effects of custodial sanctions, including imprisonment, on reoffending. Although developing slowly at first and with variable quality, this line of inquiry has produced a growing number of quasi-experimental and regression-based studies. This literature has been assessed through careful systematic reviews (Nagin, Cullen, and Jonson 2009; Loeffler and Nagin 2021) and by meta-analyses (Smith, Goggin, and Gendreau 2002; Villettaz, Killias, and Zoder 2006; Jonson 2010; Villettaz, Gillieron, and Killias 2015). Every review has reached nearly the same conclusion: *compared with noncustodial sanctions, custodial sanctions, including imprisonment, have no appreciable effect on reducing reoffending*. The studies tend to show that placing offenders in custody has a slight criminogenic effect, although this association is not sufficiently robust to argue for its certainty. In most analyses, including ours, some moderator factors may influence effect sizes, but they do not qualify the central conclusion regarding custodial sanctions.

Based on past research and the findings of this meta-analysis, the limited effects of custodial sanctions on reoffending should be viewed as *a criminological fact*. The null effects finding has been replicated repeatedly and independently. The highest quality studies reduce the criminogenic effect of custodial sanctions but do not eliminate it. This meta-analysis of a large sample of heterogeneous studies reaches the same conclusion. Narrative reviews do also. Consensus that custodial sanctions, overall, do not reduce reoffending is universal.

Calling the null effects finding a criminological fact is not an attribution of sacred status. Facts in science are based on the available literature. When the literature is slim, caution is advised. As shown vividly by Ritchie (2020) in *Science Fictions*, many claims about empirical reality do not replicate and are eventually renounced. Long ago, Merton (1942, p. 126) highlighted the importance of the scientific norm of “organized skepticism”—of the community of scholars scrutinizing assertions and suspending “judgment until ‘the facts are at hand.’” In the current case, however, scrutiny has occurred and the facts are at hand. The literature is large and its conclusions are consistent. Unless prisons and other custodial settings change their nature, there is no reason to expect that a new generation of studies will

reveal their crime-reducing effects. Although we acknowledge that empirical claims are always open to revision, if not falsification, we believe the point has been reached at which custodial sanctions as a behavioral intervention can be adjudged—using the rating scheme employed by the National Institute of Justice’s Crime Solutions—as having “no effects” (see <https://crimesolutions.ojp.gov/rated-programs>).

B. Policy Implications

Imprisonment can be justified on the grounds of just deserts and incapacitation, but the criminological fact of a null effect for custodial sanctions undermines any justification based on specific deterrence. In a time of evidence-based corrections, those favoring prisons on this basis are embracing a policy, with substantial economic and social costs, that has no demonstrable effects on reoffending. The research on custodial effects is thus salient in providing critics of incarceration with data showing that a key rationale for locking people up is empirically invalid. Advocates of custodial sanctions are in the uncomfortable position of defending something that the existing evidence concludes is ineffective (Cullen, Jonson, and Nagin 2011).

A pernicious aspect of prison policy in past decades is that the immiseration of inmates was trumpeted as a means both to exact retribution (making prisoners suffer) and to increase the pains that deliver the lesson that crime does not pay. Prisons as a deterrent requires no positive action. If prisons are crowded, unsafe, and unhealthy, the accumulation of misery makes their burdens more intense. This thinking rationalized deliberate efforts to push the policy of “austere” or “no frills” prisons (Applegate 2001). Although limited, research shows that harsh or painful prison conditions are not associated with reductions in reoffending and, if anything, are criminogenic (Chen and Shapiro 2007; Drago, Galbiati, and Vertova 2011; Listwan et al. 2013; Mastrobuoni and Terlizzese 2018). Punitive custodial conditions cannot be justified on crime-savings grounds (Cullen, Jonson, and Nagin 2011). As Durlauf and Nagin (2011) show, the core engine underlying effective deterrence is the certainty and not the severity of punishment—a stubborn reality that argues against the continued overuse and extensive financial investment in imprisonment (see also Chalfin and McCrary 2017).

A common response to the null effects finding is to call for research on the mechanisms that might cause some inmates to become more prosocial and others less so (Nagin, Cullen, and Jonson 2009; Mears, Cochran, and

Cullen 2015; Loeffler and Nagin 2021). Moderator analysis does this on a broad level, seeing, for example, if sanction effects vary by factors such as age (juveniles vs. adults) or risk (low vs. high) categories. These analyses have not yielded consistent findings. Furthermore, to unlock the black box of prison effects on individual offenders, it would be necessary to conduct, in essence, a life-course study of inmates from the time they enter prison to the time they complete a period of community supervision. Such a study would need to measure factors associated with offender change in the literature, including cognitive, motivational, or identity transformations (Maruna 2001; Paternoster and Bushway 2009; Petrich 2020), acquiring social bonds (Sampson and Laub 1993), and relinquishing antisocial attitudes and associates (Bonta and Andrews 2017). From a policy perspective, this knowledge might provide guidance on what prison programs and practices to employ while offenders are in custody.

On a broader level, a more transformative policy approach appears warranted. If the past is the best predictor of the future, there is no reason to believe that custodial settings will produce different effects unless they are fundamentally changed. More of the same will produce more of the same, which has been demonstrated by consistent findings reported in literature reviews and meta-analyses over the past two decades. Informed by the findings of the Stanford Prison Experiment (Zimbardo 2007), one position is that total institutions are inherently inhumane and coercive. They are not capable of inspiring the better angels of anyone—the kept or their keepers. Null effects, or worse, are inevitable. The alternative view is that, as with all organizations, management matters. Prisons can be “governed” well or poorly, and they can achieve goals if designed to do so (DiIulio 1987).

As Rothman (1971) detailed in his classic *Discovery of the Asylum*, the inventors of the American prison believed that if the internal workings of the prison could be designed perfectly—either as a solitary or congregate-silent system—inmates would be transformed into law-abiding citizens. Alas, the “penitentiary” was based on the flawed correctional theory that offender resistance to worldly temptations could be strengthened by forced isolation from corrupting influences in a context of hard work and religious influence. But the underlying vision of these early reformers had merit: the key to changing offenders is creating an institution intended and organized to achieve this outcome.

In this regard, two flaws inhibit the capacity of the modern correctional institution to reduce reoffending: intended goals and organizational

design. First, despite research showing that correctional workers—from wardens to correctional staff and new recruits—support rehabilitation (Cullen, Lutze, et al. 1989; Cullen, Latessa, et al. 1993; Sundt and Cullen 2002; Burton et al. 2021), these sentiments are not translated into a shared organizational goal. In particular, achieving a reduction in recidivism is not monitored or incentivized (Cullen, Jonson, and Eck 2014). In recent decades, police departments seeking to decrease crime events implemented crime mapping and statistical systems to measure fluctuations in offending and to hold managers accountable for improved performance (see, e.g., Weisburd et al. 2003). In corrections, however, no similar movement has materialized. Performance reviews of wardens and staff do not take account of institutional reoffending rates. Custodial facilities, especially prisons, are generally not evaluated for their effectiveness in changing inmate behavior. Rhetoric aside, reducing reoffending is not the intended goal of correctional institutions. It should be—if this outcome is to be attained.

Second, within community corrections, some agencies embracing the “RNR Model” (Bonta and Andrews 2017) have tried to create an organization capable of rehabilitating supervisees. A key tool is the Correctional Program Assessment Inventory (CPAI), which is a multifaceted assessment tool comprising a series of surveys or checklists that evaluators use to identify an agency’s adherence to the principles of effective intervention (Bonta and Andrews 2017). Research shows that reductions in recidivism are positively associated with scores on the CPAI (Lowenkamp, Latessa, and Smith 2006; Lowenkamp et al. 2010; Bonta and Andrews 2017). The implications for prisons and custodial settings are clear. Drawing on the evidence-based treatment theory informing the CPAI, correctional organizations should be redesigned to become people-changing institutions. This would include repeated assessment of offender risk levels, use of effective treatment modalities, building quality relations between staff and inmates, training staff in techniques to reinforce prosocial attitudes and behavior, providing released prisoners with systematic after-care, continual monitoring and evaluation of staff and the organization, and creating an organizational culture marked by concern for ethical values and for the use of core correctional practices (Bonta and Andrews 2017).

An immediate objection is that these reforms are too costly. Three responses are merited. First, many practices are not expensive but simply require greater professionalism. Effective counseling sessions lasting an hour are no more expensive than ineffective counseling sessions lasting an hour. Similarly, interacting with prisoners using cognitive behavior techniques

costs no more than interacting with prisoners coercively and ineffectively. Second, with internet access, much inmate assessment and staff training can be conducted virtually and at low expense. Such services can be delivered from centralized locations, either from within departments of corrections or from universities, across multiple institutions. Third and most important, doing more of the same with dismal results is indefensible. The opportunity costs of failing to reduce reoffending are enormous: prisoners return to crime and often to prison, and citizens are victimized in minor and serious ways. These harms are potentially preventable. Prisoner lives should matter—for their benefit and ours.

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EXHIBIT 41

CRAIG HALL

March 26, 2019

IN THE UNITED STATES DISTRICT COURT
NORTHERN DISTRICT OF CALIFORNIA
SAN JOSE DIVISION

SECURITIES AND EXCHANGE)
COMMISSION,)
)
Plaintiff,)
) Case No. 5:18-cv-01603-EJD
vs.)
)
RAMESH "SUNNY" BALWANI,)
)
Defendant.)

VIDEOTAPED ORAL DEPOSITION OF
CRAIG HALL
March 26, 2019
CLS JOB NO. 97453

VIDEOTAPED ORAL DEPOSITION OF CRAIG HALL, produced as
a witness at the instance of the Defendant, and duly
sworn, was taken in the above-styled and numbered cause
on March 26, 2019, from 9:03 a.m. to 11:51 a.m., before
Rebecca A. Graziano, CSR, RPR, CRR, in and for the State
of Texas, reported by machine shorthand, at the offices
of Reese Marketos, LLP, 750 North St. Paul Street,
Suite 600, Dallas, Texas, pursuant to the Federal Rules
of Civil Procedure and the provisions stated on the
record.

10:19:43 1 of 2013.

10:19:44 2 Q Okay. And we're going to talk about those.

10:19:45 3 A Yeah.

10:19:46 4 Q I believe one was December 20th, 2013; is that
10:19:46 5 right?

10:19:49 6 A That's right.

10:19:50 7 Q And one was December 22nd, 2013?

10:19:52 8 A That's right.

10:19:52 9 Q Okay. Do you remember any other contact with
10:19:56 10 Elizabeth Holmes regarding the 2013 investment?

10:19:59 11 A I don't.

10:20:01 12 Q Did you ever have any contact or communication
10:20:04 13 with my client, Sunny Balwani?

10:20:07 14 A No.

10:20:07 15 Q Did you ever meet him?

10:20:08 16 A No.

10:20:08 17 Q Did you know anything about him?

10:20:09 18 A Well, first of all, let me say, to my
10:20:14 19 recollection -- I've thought about it -- I may have met
10:20:18 20 him at the company when I was there, but I don't -- I
10:20:21 21 don't remember him, and I don't know anything about him
10:20:23 22 other than a little bit that I've read, and I have -- no.
10:20:27 23 I have no knowledge of him.

10:20:28 24 Q When did you visit the company?

10:20:30 25 A A couple different times, but I was there -- and

11:43:17 1 or media piece or whatever it was that you saw with
11:43:20 2 General Mattis?

11:43:21 3 A That's right.

11:43:21 4 Q Okay. In the December 20th call and the
11:43:27 5 December 22nd call, was, to your knowledge, Sunny Balwani
11:43:31 6 on those calls?

11:43:31 7 A I don't believe so.

11:43:32 8 Q Okay. And I think you've said this before, but
11:43:36 9 just to confirm, have you ever spoken to Mr. Balwani?

11:43:38 10 A No. I don't believe so.

11:43:39 11 Q Okay.

11:44:02 12 MR. COOPERSMITH: Okay. I have no further
11:44:03 13 questions.

11:44:03 14 EXAMINATION

11:44:03 15 QUESTIONS BY MR. KATZ:

11:44:04 16 Q I have just a few questions, Mr. Hall. Again,
11:44:07 17 I'm Marc Katz from the SEC.

11:44:10 18 If you could please take what has been
11:44:11 19 marked as Exhibit 51 and 52. They're two letters dated
11:44:19 20 January 3rd, 2014. Tell me when you have those.

11:44:24 21 A I'm looking for 52.

11:44:29 22 MR. REESE: You said 51 and 52?

11:44:31 23 THE WITNESS: Yeah.

11:44:33 24 MR. KATZ: Yes, please.

11:44:34 25 MR. REESE: Right here.